THE

HISTORY

AND

PRESENT STATE

OF

ELECTRICITY,

WITH

ORIGINAL EXPERIMENTS,

By JOSEPH PRIESTLEY, LL.D. F.R.S.

THE THIRD EDITION,

CORRECTED AND ENLARGED.

Causa latet, vis est notissima. OVID.

VOL. II.

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THE

HISTORY

AND PRESENT STATE OF

ELECTRICITY.

PART II.

A

SERIES OF PROPOSITIONS,

COMPRISING ALL THE

GENERAL PROPERTIES OF ELECTRICIY.

AFTER tracing, at large, the progrefs of all the difcoveries relating to electricity, and giving an hiftorical account of them, in the order in which they were made; it will, probably, be no difagreeable repetition, if I give, at the clofe of it, a SERIES OF PROPOSITIONS, comprifing all the general properties of electricity, drawn up in as fuccinct a manner as poffible. And, notwithftanding the large detail which has been made, it will be found, that a few propofitions are fufficient to comprife almoft all that we know of the fubject.

THIS circumftance may be regarded as a demonftration of the real progrefs that has been made in this ſcience And as this progreſs advances, and the hiſtory enlarges, paradoxical as the aſſertion may ſeem, this part may be expected to contract itſelf in the ſame proportion. For the more we know of any ſcience, the greater number of particular propoſitions are we able to reſolve into general ones; and, conſequently, within narrower bounds ſhall we be able to reduce its principles.

I MIGHT have made this part of my work much ſhorter, even in the preſent ſtate of the ſcience, if I would have admitted into it any thing theoretical; but I have carefully avoided the principles of any theory, even the moſt probable, and the neareſt to being perfectly aſcertained, in this ſeries of propoſitions; in which I propoſe to comprehend only *known facts*; that my younger readers may carefully diſtinguiſh between *fact* and *theory*; things which are too often confounded.

I HAVE not, in this part of my work, deſcended to any *minutiæ*, in the deſcription of electrical appearances, becauſe they have been entered into before, and a repetition of them would have been tedious. At the ſame time, I think it will be found, upon examination, that I have not omitted to take notice of any diſcovery of importance. I have alſo introduced into it the definition of all the moſt neceſſary *technical terms*; that this part of the work might ſerve as a methodical introduction, to thoſe who are beginning the ſtudy of electricity, and deſire a general knowledge of the firſt elements of the ſcience, before they enter upon the detail of particulars, which will be beſt learned afterwards, from the hiſtory *.

By *electricity*, in the following propoſitions, I would be underſtood to mean, only thoſe *effects* which will be called electrical; or elſe the *unknown cauſe* of thoſe effects, uſing the term, as we uſe the letters x and y in Algebra.

All known ſubſtances are diſtributed by electricians into two ſorts. Thoſe of one ſort are termed *electrics*, or *non-conductors*; and thoſe of the other *non-electrics*, or *conductors of electricity*.

Metals of all kinds, together with ſemi-metals, and water are conductors. So alſo is charcoal, and other ſubſtances of a ſimilar nature, as will be ſhewn at large in the laſt part of this work. All other ſubſtances, whether mineral, vegetable, or animal, are non-conductors. But many of theſe when they are made very hot, as glaſs, roſin, baked wood, and, perhaps, all the reſt on which the experiment can be made in this ſtate, are conductors of electricity.

All bodies, however, though in the ſame ſtate of heat and cold, are not equally perfect electrics, or perfect conductors. Vegetable

* Leſt this part of the work ſhould not prove a ſufficient *introduction to the ſtudy of electricity*, I have ſince publiſhed a ſmall piece with this title. It contains a more familiar explication of the fundamental principles of electricity, mixing theory with facts, and illuſtrating, chiefly, thoſe experiments which are the moſt entertaining.

and animal ſubſtances, for inſtance, in their natural ſtate, are ſeldom perfect electrics, on account of the moiſture that is contained in them. And, independent of moiſture, there is probably a gradation in all ſubſtances, from the moſt perfect conductors to the moſt perfect non-conductors of electricity.

It is the property of all kinds of electrics, that when they are rubbed by bodies differing from themſelves (in roughneſs or ſmoothneſs chiefly) to attract light bodies of all kinds which are preſented to them; to exhibit an appearance of light (which is very viſible in the dark) attended with a ſnapping noiſe, upon the approach of any conductor; and, if the noſtrils be preſented, they are affected with a ſmell like that of phoſphorus.

An electric ſubſtance exhibiting theſe appearances, is ſaid to be *excited*, and ſome of them, particularly the tourmalin, are excited by heating and cooling, as well as by rubbing.

It is neceſſary, however, to a conſiderable excitation of any electric, that the ſubſtance againſt which it is rubbed (uſually called the *rubber)* have a communication with the earth, or bodies abounding with electricity, by means of conductors; for if the rubber be *inſulated*, that is, if it be cut off from all communication with the earth by means of of electrics, the friction has but little effect.

When inſulated bodies have been attracted by, and brought into contact with any excited electric, they begin to be repelled by it, and

and alſo to repel one another: nor will they be attracted again, till they have been in contact with ſome conductor communicating with the earth; but, after this, they will be attracted as at firſt.

If conductors be inſulated, electric powers may be *communicated* to them by the approach of excited electrics. They will then attract light bodies, and give ſparks, attended with a ſnapping noiſe, like the electrics themſelves. But there is this difference between excited and communicated electricity, that a conductor to which electricity has been communicated parts with its whole power at once, on the contact of a conductor communicating with the earth; whereas an excited electric, in the ſame circumſtances, loſes its electricity only partially; it being diſcharged only from the part which was actually touched by the conductor, or thoſe in the neighbourhood of it; ſo that the ſpark of electric fire is not ſo denſe, nor the exploſion made by parting with it ſo loud, from excited as from communicated electricity.

Electric ſubſtances brought into contact with excited electrics, will not deſtroy their electricity; whence it is that they are called non-conductors, becauſe they will not convey or *conduct* away whatever is the cauſe of electric appearances in bodies.

When electricity is ſtrongly communicated to inſulated animal bodies, the pulſe is quickened, and perſpiration increaſed; and, if they receive, or part with their electricity on a ſudden, a painful ſenſation is felt at the place of communication.

The growth of vegetables is quickened by electricity.

No electric can be excited without producing electric appearances in the body with which it is excited, provided that body be inſulated. For this inſulated rubber will attract light bodies, give ſparks, and make a ſnapping noiſe upon the approach of a conductor, as well as the excited electric.

If an inſulated conductor be pointed, or if a pointed conductor communicating with the earth be held pretty near it, little or no electric appearance will be exhibited; only a light will appear at each of the points, during the act of excitation, and a current of air will be ſenſible from off them both.

These two electricities, viz. that of the electric itſelf, and that of the rubber, though ſimilar to, are the reverſe of one another. A body attracted by the one will be repelled by the other, and they will attract, and in all reſpects act upon one another more ſenſibly than upon other bodies; ſo that two pieces of glaſs or ſilk, poſſeſſed of contrary electricities, will cohere firmly together, and require a conſiderable force to ſeparate them.

These two electricities, having been firſt diſcovered by producing one of them from glaſs, and the other from amber, ſealing-wax, ſulphur, roſin, &c. firſt obtained the names of *vitreous* and *reſinous electricity*; and it being afterwards imagined that one of them was

was a redundancy, and the other a deficiency of a ſuppoſed electric fluid, the former (viz. that which is produced from the friction of ſmooth glaſs tubes or globes by the human hand, or a common leathern rubber) obtained the name of *poſitive*; and the latter (viz. that which is produced from the friction of ſticks or globes of ſulphur, &c. or collected from the rubber of a glaſs globe above mentioned) that of *negative* electricity: and theſe terms are now principally in uſe.

If a conductor, not inſulated, be brought within the *atmoſphere*, that is, the ſphere of action, of any electrified body, it acquires the electricity oppoſite to that of the electrified body; and the nearer it is brought, the ſtronger oppoſite electricity doth it acquire, till the one receive a ſpark from the other, and then the electricity of both will be diſcharged.

The electric ſubſtance which ſeparates the two conductors, poſſeſſing theſe two oppoſite kinds of electricity, is ſaid to be *charged*. Plates of glaſs are the moſt convenient for this purpoſe, and the thinner the plate, the greater charge it is capable of holding. The conductors contiguous to each ſide of the glaſs are called their *coating*.

Agreeable to the above-mentioned general principle, it is neceſſary, that one ſide of the charged glaſs have a communication with the rubber, while the other receives the electricity from the conductor, or with the conductor, while the other receives from the rubber.

It follows alſo, that the two ſides of the plate thus charged are always poſſeſſed of the two oppoſite electricities; that ſide which communicates with the excited electric having the electricity of the electric, and that which communicates with the rubber, that of the rubber.

There is, conſequently, a very eager attraction between theſe two electricities with which the different ſides of the plate are charged; and, when a proper communication is made by means of conductors, a flaſh of electric light, attended with a report (which is greater or leſs in proportion to the quantity of electricity communicated to them, and the goodneſs of the conductors) is perceived between them, and the electricity of both ſides is thereby diſcharged.

The ſubſtance of the glaſs itſelf, in or upon which theſe electricities exiſt, is impervious to electricity, and does not permit them to unite; but if they be very ſtrong, and the plate of glaſs very thin, they will force a paſſage through the glaſs. This, however, always breaks the glaſs, and renders it incapable of another charge.

The flaſh of light, together with the exploſion between the two oppoſite ſides of a charged electric, is generally called the *electric ſhock*, on account of the diſagreeable ſenſation it gives any animal, whoſe body is made uſe ufe of to form the communication between them.

THIS electric fhock is always found to perform the circuit from one fide of the charged glafs to the other by the fhorteft paffage, through the beft conductors. Common communicated electricity alfo obferves the fame rule, in its tranfmiffion from one body to another.

IT has not been found that the electric fhock, takes up the leaft fenfible fpace of time in being tranfmitted to the greateft diftances.

THE electric fhock, as alfo the common electric fpark, difplaces the air through which it paffes; and if its paffage from conductor to conductor be interrupted by non-conductors of a moderate thicknefs, it will rend and tear them in its paffage; and in fuch a manner as to exhibit the appearance of a fudden expanfion of the air about the center of the fhock.

IF an electric fhock, or ftrong fpark, be made to pafs through, or over the belly of a mufcle, it forces it to contract as in a convulfion.

IF a ftrong fhock be fent through a fmall animal body, it will often deprive it inftantly of life.

WHEN the electric fhock is very ftrong, it will give polarity to magnetic needles, and fometimes it reverfes their poles.

GREAT fhocks, by which animals are killed, are faid to haften putrefaction.

ELEC-

Electricity and lightning are, in all reſpects, the ſame thing. Every effect of lightning may be imitated by electricity, and every experiment in electricity may be made with lightning, brought down from the clouds, by means of inſulated pointed rods of metal.

PART

PART III.

THEORIES OF ELECTRICITY.

SECTION I.

OF PHILOSOPHICAL THEORIES IN GENERAL, AND THE THEORIES OF ELECTRICITY PRECEDING THAT OF DR. FRANKLIN.

ONE of the moſt intimate of all aſſociations in the human mind is that of *cauſe* and *effect*. They ſuggeſt one another with the utmoſt readineſs upon all occaſions; ſo that it is almoſt impoſſible to contemplate the one, without having ſome idea of, or forming ſome conjecture about the other. In viewing the works of nature, we neceſſarily become firſt acquainted with appearances or effects. We naturally attend to the circumſtances in which ſuch appearances always ariſe, and cannot help conſidering them as the cauſes of thoſe appearances. Then, conſidering theſe circumſtances themſelves as new appearances, we are deſirous of tracing out other circumſtances that gave birth to them. Thus, conſtantly aſcending in this chain of cauſes

causes and effects, we are led, at last, to the first cause of all: and then we consider all secondary and inferior causes, as nothing more than the various methods in which the supreme cause acts, in order to bring about his great designs.

In all science, we first ascend from particular to general. For nature exhibits nothing but particulars; and all general propositions, as well as general terms, are artificial things, being contrived for the ease of our conception and memory; in order to comprehend things clearly, and to comprise as much knowledge as possible in the smallest compass. It is no wonder then that we take pleasure in this process. Besides, we actually see in nature a vast variety of effects proceeding from the same general principles, operating in different circumstances; so that judging from appearances, that nature is every where uniform with itself, we are led, by analogy, to expect the same in all cases, and think it an argument in favour of any system, if it exhibits a variety of effects springing from a few causes. For such variety in effects, and such simplicity in causes, we generally see in nature.

Having discovered the cause of any appearance, it is the business of philosophy to trace it in all its effects, and to predict other similar appearances from similar previous situations of things. By this means, the true philosopher, knowing what will be the result of putting every thing, which the present system exhibits, into every variety of circum-

stances,

ſtances, is maſter of all the powers of nature, and can apply them to all the uſeful purpoſes of life. Thus does *knowledge*, as Lord Bacon obſerves, become *power*; and thus is the philoſopher capable of providing, in a more effectual manner, both for his own happineſs and for that of others; and thereby of approving himſelf a good citizen, and an uſeful member of ſociety.

It is obvious, from this general view of the buſineſs of philoſophy, that, in order to trace thoſe circumſtances in which any appearance in nature is certainly and invariably produced, it is chiefly uſeful to obſerve what there is *in common* in the circumſtances attending ſimilar appearances: for on thoſe common circumſtances, all that is common in the appearances muſt depend. And the eaſieſt poſſible method, by which we can trace out the connection of cauſes and effects in nature, is to begin with comparing thoſe appearances which are moſt ſimilar, where the difference conſiſts in a ſingle circumſtance; the whole effect of which, in different appearances, is thereby perfectly known. And when we have, by this means, noted the whole effect of all the ſeparate circumſtances and ſituations of things, we can eaſily judge of their effect in all poſſible combinations.

Hence *analogy* is our beſt guide in all philoſophical inveſtigations; and all diſcoveries, which were not made by mere accident, have been made by the help of it. We obſerve a complex appearance, attended with a propor-

tionable

tionable variety of circumftances and fituations. We alfo fee another appearance, in fome refpects fimilar, in others diffimilar; the circumftances being, likewife, fimilar and diffimilar in the fame proportion; or we purpofely vary the circumftances of the former appearance, and obferve what difference it occafions. But, unlefs there be a very great analogy, or fimilarity, between them, fo that the influence of a fingle circumftance, or of a few circumftances, can be traced feparately, no probable judgment can be formed of their real operation.

BUT in all this procefs, a man who acts from defign, and not abfolutely at random, would never think of trying the influence of any circumftance in an appearance, unlefs, from fome other analogies in nature, more or lefs perfect, he had formed fome idea what its influence would probably be; at leaft, he muft, from analogies in nature previoufly obferved, have formed an idea of feveral poffible confequences, and try which of them will really follow. That is, in other words, every experiment, in which there is any defign, is made to afcertain fome *hypothefis*. For an hypothefis is nothing more than a preconceived idea of an event, as fuppofed to arife from certain circumftances, which muft have been imagined to have produced the fame, or a fimilar effect, upon other occafions. An hypothefis abfolutely verified ceafes to be termed fuch, and is confidered as a fact; though, when it has long been in an hypothetical ftate,

it

it may continue to be called, occafionally, by the fame name.

The only danger in the ufe of hypothefes arifes from making this tranfition too foon. And when an hypothefis is no longer confidered as a mere probable fuppofition, but a real fact; a philofopher not only acquiefces in it, and thereby miftakes the caufe of one particular appearance; but, by its analogies, he miftakes the caufe of other appearances too, and is led into a whole fyftem of error. A philofopher who has been long attached to a favourite hypothefis, and efpecially if he have diftinguifhed himfelf by his ingenuity in difcovering or purfuing it, will not, fometimes, be convinced of its falfity by the plaineft evidence of fact. Thus both himfelf, and all his followers, are put upon falfe purfuits, and feem determined to warp the whole courfe of nature, to fuit their manner of conceiving of its operations.

But, provided philofophers can be upon their guard againft this fpecies of vanity (which muft be owned to be very tempting) and againft the obftinacy which is the confequence of it; hypothefes, and even a great variety of them, are certainly very promifing circumftances to philofophical difcoveries. Hypothefes, while they are confidered merely as fuch, lead perfons to try a variety of experiments, in order to afcertain them. In thefe experiments, new facts generally arife. Thefe new facts ferve to correct the hypothefis which gave occafion to them. The theory, thus cor-

corrected, serves to discover more new facts; which, as before, bring the theory still nearer to the truth. In this progressive state, or method of approximation, things continue; till, by degrees, we may hope that we shall have discovered all the facts, and have formed a perfect theory of them. By this perfect theory, I mean a system of propositions, accurately defining all the circumstances of every appearance, the separate effect of each circumstance, and the manner of its operation.

I have dwelt so long upon this subject, because I apprehend, that electricians have generally been too much attached to their several theories, so as to have retarded the progress of real discoveries. Indeed, no other part of the whole compass of philosophy affords so fine a scene for ingenious speculation. Here the imagination may have full play, in conceiving of the manner in which an invisible agent produces an almost infinite variety of visible effects. As the agent is invisible, every philosopher is at liberty to make it whatever he pleases, and ascribe to it such properties and powers as are most convenient for his purpose. And, indeed, if he can frame this theory so as really to suit all the facts, it has all the evidence of truth that the nature of things can admit.

With the first electricians, electrical attraction was performed by means of *unctuous effluvia* emitted by the excited electric. These were supposed to fasten upon all bodies in their way, and to carry back with them all that were not too heavy. For, in that age of philoſophy, all effluvia were ſuppoſed to return to the bodies from which they were emitted; ſince no perſon could, otherwiſe, account for the ſubſtance not being ſenſibly waſted by the conſtant emiſſion. When theſe light bodies, on which the unctuous effluvia had faſtened, were arrived at the excited electric, a freſh emiſſion of the effluvia was ſuppoſed to carry them back again. But this effect of the effluvia was not thought of, till electrical repulſion had been ſufficiently obſerved.

When the Newtonian philoſophy had made ſome progreſs, and the extreme ſubtilty of light, and other effluvia of bodies, was demonſtrated; ſo that philoſophers were under no apprehenſion of bodies being waſted by continual emiſſion, the doctrine of *the return of the effluvia* was univerſally given up as no longer neceſſary, and they were obliged to acquieſce in the unknown principles of attraction and repulſion, as ſuppoſed to be properties of certain bodies, communicated to them by the Divine Being, the mechanical cauſe of which they ſcarce attempted to explain.

When Mr. Du Fay diſcovered the two oppoſite ſpecies of electricity, which he termed the *vitreous* and *reſinous* electricity, he neceſſarily formed the idea of *two diſtinct electric fluids*, repulſive with reſpect to themſelves, and attractive of one another. But he had no idea of both ſpecies being actually concerned in every electrical operation, and that glaſs or roſin alone always produced them both. This theory, therefore, was as ſimple in its application as the other.

While nothing more was known of electricity but attraction and repulſion, this general theory was ſufficient. The general attraction of all bodies to all bodies was called (and by ſome abſurdly enough ſuppoſed to be accounted for by) gravitation, and many ſuperficial philoſophers thought they had given a very good account of electricity, coheſion, and magnetiſm, by calling them particular ſpecies of attraction peculiar to certain bodies.

But when electricity began to ſhow itſelf in a greater variety of appearances, and to make itſelf ſenſible to the ſmell, the ſight, the touch, and the hearing: when bodies were not only attracted and repelled, but made to emit ſtrong ſparks of fire, attended with a conſiderable noiſe, a painful ſenſation, and a ſtrong phoſphoreal ſmell; electricians were obliged to make their ſyſtems more complex, in proportion as the facts were ſo. It was then generally ſuppoſed, that the matter of the electric fluid was the ſame with the chymical principle of fire; though ſome thought it was a fluid *ſui generis*, which very much reſembled that of fire; and others, with Mr. Boulanger at their head, thought that the electric fluid was nothing more than the finer parts of the atmoſphere, which crouded upon the

the ſurfaces of electric bodies, when the groſſer parts had been driven away by the friction of the rubber.

THE great difficulty common to all theſe theories was to aſcertain the direction of the electric matter. It is no wonder that, when electrical appearances were firſt obſerved, all electric powers were ſuppoſed to reſide in, and therefore to proceed from the excited electric. Conſequently, the electric ſpark was firſt imagined to be darted from the electrified body towards any conductor that was preſented to it. It was never imagined there could be any difference in this reſpect whether it was amber, glaſs, ſealing-wax, or any thing elſe that was excited. Nothing was thought to be more evident to the ſenſes than this progreſs of the electric matter: what then muſt have been the aſtoniſhment of all electricians, when they firſt obſerved electric appearances at an inſulated rubber; at the ſame time that it was demonſtrated, that the action of the rubber did not *produce*, but only *collect* the electric fluid.

IN this caſe, the current could not have been ſuppoſed to flow both from the conductor and the rubber; and yet the firſt appearances were the ſame. To provide a ſupply of the electric matter, they were obliged to ſuppoſe that, notwithſtanding appearances were nearly the ſame, the electric fluid was really received by the electrified body in the one caſe, and emitted by it in the other. But now, being obliged to give up the argument for the manner of its progreſs from the evidence of ſight, they were at a loſs whether, in the uſual method of electrifying by excited glaſs, the fluid proceeded from the rubber to the conductor, or from the conductor to the rubber; and nothing was found to obviate theſe difficulties till an excellent theory of poſitive and negative electricity was ſuggeſted by Dr. Watſon, and digeſted and illuſtrated by Dr. Franklin.

It was ſoon found, that the electricity, at the rubber was the reverſe of that at the conductor, and in all reſpects the ſame with that which had before been produced by the friction of ſealing-wax, ſulphur, roſin, &c. Seeing, therefore, that both the electricities, as they had heretofore been called, were produced at the ſame time, by one and the ſame electric, and by the ſame friction, all electricians, and among the reſt Mr. Du Fay himſelf, concluded, that they were both modifications of one and the ſame fluid; and the old doctrine of the different electricities was univerſally diſcarded.

The accidental diſcovery of the Leyden phial moſt clearly demonſtrated the imperfection of all the theories preceding that of poſitive and negative electricity, by exhibiting an aſtoniſhing appearance, which no electricians, with the help of any theory, could have foreſeen, and of which they could have formed no idea, *a priori*.

Upon this great event, new theories of electricity multiplied apace, ſo that it would be to no purpofe to enumerate them all. Indeed many of them were no more than the beings of a day. For no fooner were they ftarted, but the authors themfelves, upon the appearance of fome new fact, faw reafon to new model, or entirely reject them. I fhall, therefore, content myfelf with giving the outlines of fome of the principal theories of electricity, which have their adherents at prefent, without confidering whether they took their rife before or after this difcovery.

With fome, and particularly Mr Wilfon, the chief agent in all electrical operations is Sir Ifaac Newton's ether; which is more or lefs denfe in all bodies, in proportion to the fmallnefs of their pores, except that it is much denfer in fulphureous and unctuous bodies *. To this ether are afcribed the principal phenomena of attraction and repulfion, whereas the light, the fmell, and other fenfible qualities of the electric fluid are referred to the groffer particles of bodies, driven from them by the forcible action of this ether. Many phenomena in electricity are alfo attempted to be explained by means of a fubtile medium, at the furface of all bodies, which is the caufe of the refraction and reflection of the rays of light, and alfo refift the entrance and exit of this ether †. This medium, he fays, extends to a fmall diftance from the body, and is of the fame nature with what is called the electric fluid. On the furface of

* Wilfon's Differtation, p. 5. † Hoadley and Wilfon, p. 55.

conductors this medium is rare, and easily admits the passage of the electric fluid; whereas on the surface of electrics it is dense, and resists it This medium is rarefied by heat, which converts non-conductors into conductors *. On this theory I shall make no particular remarks, because I cannot say that I clearly comprehend it.

But the far greater number of philosophers suppose, and with the greatest probability, that there is a fluid *sui generis* principally concerned in the business of electricity. They seem, however, though perhaps without reason, entirely to overlook Sir Isaac Newton's ether; or if they do not suppose it to be wholly unconcerned, they allow it only a secondary and subordinate part to act in this drama. And among those who suppose a fluid *sui generis*, there is a great diversity of opinions about the mode of its existence, and the manner of its operation.

The ingenious Abbé Nollet, whose theory has been more the subject of debate than all the other theories before Dr. Franklin's, supposes that, in all electrical operations, the fluid is thrown into two opposite motions; that the *affluence* of this matter drives all light bodies before it, by impulse, upon the electrified body, and its *effluence* carries them back again. But he seems very much embarrassed in accounting for facts where both these currents must be considered, at the same

* Hoadley and Wilson, p. 78.

time

time that he is obliged to find expedients to prevent their impeding the effects of each other. To obviate this great difficulty, he ſuppoſes, that every excited electric, and likewiſe every body to which electricity is communicated, has two orders of pores, one for the emiſſion of the effluvia, and the other for the reception of them. A man of leſs ingenuity than the Abbé could not have maintained himſelf in ſuch a theory as this; but, with his fund of invention, he was never at a loſs for reſources upon all emergencies, and in his laſt publication appears to be as zealous for this ſtrange hypotheſis as at the firſt.

He more than once requeſted a deputation of the members of the Academy of Sciences, to be witneſſes of ſome experiments, in which, he thought, there was a viſible effluence of the electrical effluvia from the conductor, both to the globe at one of its extremities, and to any non-electric preſented to it at the other; and their teſtimony was ſigned and regiſtered in proper form *. But it does not ſeem to the honour of Mr. Nollet, or thoſe gentlemen of the Academy, to be ſo very poſitive in a matter which does not admit of the evidence of ſenſe.

The Abbé's confidence upon this ſubject is very remarkable. Theſe effects, ſays he, well conſidered, and reviewed a thouſand times, in the courſe of thirty years, in which

* Leçons de Phyſique, p. 368. 395.

I have applied to electricity, make me ſay with confidence, that thoſe pencils of rays are currents of electric matter, which fly from the conductor towards the excited globe. This is ſo evident, that I would freely appeal to the ocular teſtimony of any unprejudiced perſon, who ſhould ſee the experiments which I have recited. But, ſays he, the fact in queſtion is contrary to a ſyſtem of electricity, which ſome perſons perſiſt in maintaining. They have the aſſurance to tell me, that the matter of the luminous pencil, in my experiment, moves in a direction quite oppoſite to that which I ſuppoſe; that it proceeds from the excited globe, and is from thence thrown upon any non-electric within its reach *. In another place, he ſays, that the principle of ſimultaneous effluences and affluences is by no means a *ſyſtem*, but a *fact* well proved †.

The Abbé Nollet propoſes an hypotheſis to explain the difference between common electricity and the electric ſhock. All the effects of common electricity, he ſays, plainly ſhow, that the electric matter is animated with a progreſſive motion, which really carries it forwards; whereas the remarkable caſe of the electric ſhock appears to be an inſtantaneous percuſſion, which the contiguous parts of the ſame matter communicate to one another, without being diſplaced. Sound and wind, he ſays, are motions of the air; but would a philoſopher, be permitted to take the one for

* Leçons de Phyſique, p. 363.

† Lettres ſur l'Electricite, p. 98.

the

the other, in meaſuring their velocity or extent. But this compariſon is by no means juſt *.

It muſt be acknowledged, that far the greater part of the Abbé Nollet's arguments in favour of his doctrine of effluences and affluences are very unſatisfactory, and that his method of accounting for electrical attraction and repulſion, with other phenomena in electricity, by means of it, is more ingenious than ſolid. It is a great pity that this truly excellent philoſopher had not ſpent more time in diverſifying facts, and leſs in refining upon theory. But it is in ſome meaſure the natural fault of a diſpoſition to philoſophize.

Mr. Du Tour improves upon this hypotheſis of the Abbé Nollet, by ſuppoſing that there is a difference between the affluent and effluent current; and that the particles of the fluid are thrown into vibrations of different qualities, which makes one of theſe currents more copious than the other, according as ſulphur or glaſs is uſed. Difficult as it is to form any idea of this hypotheſis, the author appears very much attached to it, and has no doubt of its accounting for all electrical appearances.

* Leçons de Phyſique, p 293.

SECTION

SECTION II.

THE THEORY OF POSITIVE AND NEGATIVE ELECTRICITY.

THE English philoſophers, and perhaps the greater part of foreigners too, have now generally adopted the theory of *poſitive* and *negative* electricity. As this theory has been extended to almoſt all the phenomena, and is the moſt probable of any that have been hitherto propoſed to the world, I ſhall give a pretty full account of it, and ſhow how it agrees with all the propoſitions of the laſt part, to which it has hitherto been applied.

THIS theory generally goes by the name of Dr. Franklin, and there is no doubt of his right to it; but juſtice requires that I diſtinctly mention the equal, and, perhaps, prior claim of Dr. Watſon, to whom I have before ſaid it had occurred. Dr. Watſon ſhowed a ſeries of experiments to confirm the doctrine of *plus* and *minus* electricity to Martin Folkes, Eſq. then preſident, and to a great number of fellows of the Royal Society, ſo early as the beginning of the year 1747, before it was known in England that Dr. Franklin had diſcovered the ſame thing in America. See the Philoſophical Tranſactions, vol. xliv. p. 739; and vol. xlv. p. 93—101. Dr. Franklin's paper,

paper, containing the ſame diſcovery, was dated at Philadelphia, June the 1ſt, 1747.

According to this theory, all the operations of electricity depend upon one fluid *ſui generis*, extremely ſubtile and elaſtic, diſperſed through the pores of all bodies; by which the particles of it are as ſtrongly attracted, as they are repelled by one another.

When the equilibrium of this fluid in any body is not diſturbed; that is, when there is in any body neither more nor leſs of it than its natural ſhare, or than that quantity which it is capable of retaining by its own attraction, it does not diſcover itſelf to our ſenſes by any effect. The action of the rubber upon an electric diſturbs this equilibrium, occaſioning a deficiency of the fluid in one place, and a redundancy of it in another.

This equilibrium being forcibly diſturbed, the mutual repulſion of the particles of the fluid is neceſſarily exerted to reſtore it. If two bodies be both of them overcharged, the electric atmoſpheres (to adopt the ideas of all the patrons of this hypotheſis before Mr. Æpinus) repel each other, and both the bodies recede from one another to places where the fluid is leſs denſe. For, as there is ſuppoſed to be a mutual attraction between all bodies and the electric fluid, electrified bodies go along with their atmoſpheres. If both the bodies be exhauſted of their natural ſhare of this fluid, they are both attracted by the denſer fluid, exiſting either in the atmoſphere con-

contiguous to them; or in other neighbouring bodies; which occaſions them ſtill to recede from one another, as much as when they were overcharged.

SOME of the patrons of the hypotheſis of poſitive and negative electricity conceive otherwiſe of the immediate cauſe of this repulſion. They ſay that, as the denſer electric fluid, ſurrounding two bodies negatively electrified, acts equally on all ſides of thoſe bodies, it cannot occaſion their repulſion. Is not the repulſion, ſay they, owing rather to an accumulation of the electric fluid on the ſurfaces of the two bodies; which accumulation is produced by the attraction of the bodies, and the difficulty the fluid finds in entering them? This difficulty in entering is ſuppoſed to be owing, chiefly, to the *air* on the ſurface of bodies, which is probably a little condenſed there; as may appear from Mr. Canton's experiment above mentioned on the double barometer.

LASTLY, if one of the bodies have an overplus of the fluid, and the other a deficiency of it, the equilibrium is reſtored with great violence, and all electrical appearances between them are more ſtriking.

THE influence of *points* in drawing or throwing off the electric fluid has not been quite ſatisfactorily accounted for upon any hypotheſis, but it is as agreeable to this as any other. As it is evident that every electric atmoſphere meets with ſome reſiſtance, both in entering and quitting any body, what-

ever

ever be the caufe of that refiftance, it is natural to fuppofe, that it muft be leaft at the points of bodies where there are fewer particles of the body (on which the refiftance depends) oppofed to its paffage, than at the flat parts of the furface, where the refifting power of a greater number of particles is united.

The *light* which is vifible in electrical appearances is generally fuppofed to be part of the compofition of the electric fluid, which appears when it is properly agitated. But this fuppofition concerning electric light is not neceffary to the general hypothefis. It may be fuppofed, upon this as well as Mr. Wilfon's theory, that the light, and the phofphoreal fmell, in electrical experiments arife from particles of matter much groffer than the proper electric fluid, but which may be driven from bodies by its powerful action.

The *found* of an electrical explofion is certainly produced by the air being difplaced by the electrical fluid, and then fuddenly collapfing, fo as to occafion a vibration, which diffufes itfelf every way from the place where the explofion was made. For in fuch vibrations found is known to confift.

But the chief excellence of this theory of pofitive and negative electricity, and that which gave it the greateft reputation, is the eafy explication which it fuggefts of all the phenomena of the Leyden phial. This fluid is fuppofed to move with the greateft eafe in bodies which are conductors, but with extreme difficulty in electrics *per fe*; infomuch that

that glaſs is abſolutely impermeable to it. It is moreover ſuppoſed, that all electrics (and particularly glaſs) on account of the ſmallneſs of their pores, do at all times contain an exceedingly great, and always an equal quantity of this fluid; ſo that no more can be thrown into one part of any electric ſubſtance, except the ſame quantity go out at another, and the gain be exactly equal to the loſs. Theſe things being previouſly ſuppoſed, the phenomena of charging and diſcharging a plate of glaſs admit of an eaſy ſolution.

In the uſual manner of electrifying, by a ſmooth glaſs globe, all the electric matter is ſupplied by the rubber from all the bodies which communicate with it. If it be made to communicate with nothing but one of the coatings of a plate of glaſs, while the conductor communicates with the other, that ſide of the glaſs which communicates with the rubber muſt neceſſarily be exhauſted, in order to ſupply the conductor, which muſt convey the whole of it to the ſide with which it communicates. By this operation, therefore, the electric fluid becomes almoſt entirely exhauſted on one ſide of the plate, while it is as much accumulated on the other; and the diſcharge is made by the electric fluid ruſhing, as ſoon as an opportunity is given it, by means of proper conductors, from the ſide which was overloaded to that which was exhauſted.

It is not neceſſary, however, to this theory, that the very ſame individual particles of electric matter which were thrown upon one ſide of the plate, ſhould make the whole circuit of the intervening conductors, eſpecially in very great diſtances, ſo as actually to arrive at the exhauſted ſide. It may be ſufficient to ſuppoſe, as was obſerved before, that the additional quantity of fluid diſplaces and occupies the ſpace of an equal portion of the natural quantity of fluid belonging to thoſe conductors in the circuit, which lay contiguous to the charged ſide of the glaſs This diſplaced fluid may drive forwards an equal quantity of the ſame matter in the next conductors; and thus the progreſs may continue, till the exhauſted ſide of the glaſs is ſupplied by the fluid naturally exiſting in the conductors contiguous to it. In this caſe the motion of the electric fluid in an exploſion will rather reſemble the vibration of the air in ſounds, than a current of it in winds.

It will eaſily be acknowledged, that while the ſubſtance of the glaſs is ſuppoſed to contain as much as it can poſſibly hold of the electric fluid, no part of it can be forced into one of the ſides, without obliging an equal quantity to quit the other ſide; but it may be thought a difficulty upon this hypotheſis, that one of the ſides of a glaſs plate cannot be *exhauſted*, without the other receiving more than its natural ſhare, particularly as the particles of this fluid are ſuppoſed to be repulſive of one another. But it muſt be conſidered, that the attraction of the glaſs is ſufficient to retain even the large quantity of the electric fluid which

which is natural to it, againſt all attempts to withdraw it, unleſs that eager attraction can be ſatisfied by the admiſſion of an equal quantity from ſome other quarter. When this opportunity of a ſupply is given, by connecting one of the coatings with the rubber, and the other with the conductor, the two attempts to introduce more of the fluid into one of the ſides, and to take from it on the other, are made, in a manner, at the ſame inſtant. The action of the rubber tends to diſturb the equilibrium of the fluid in the glaſs, and no ſooner has a ſpark quitted one of the ſides, to go to the rubber, but it is ſupplied by the conductor on the other; and the difficulty with which theſe additional particles move in the ſubſtance of the glaſs effectually prevents its reaching the oppoſite exhauſted ſide; near as the two ſides are to one another, and eager as is the endeavour of the fluid to go whither it is ſo ſtrongly attracted.

It is not ſaid, however, but that either ſide of the glaſs may give or receive *a ſmall quantity* of the electric fluid, without altering the quantity on the oppoſite ſide. It is only a very conſiderable part of the charge that is meant, when one ſide is ſaid to be filled, while the other is exhauſted.

It is a little remarkable, that the electric fluid, in this, and in every other hypotheſis, ſhould ſo much reſemble the ether of Sir Iſaac Newton in ſome reſpects, and yet differ from it ſo eſſentially in others. The electric fluid is ſuppoſed to be, like ether, extremely ſubtile and

and elaſtic, that is, repulſive of itſelf; but, inſtead of being, like the ether, repelled by all other matter, it is ſtrongly attracted by it; ſo that, far from being, like the ether, rarer in the ſmall than in the large pores of bodies, rarer within the bodies than at their ſurfaces, and rarer at their ſurfaces than at any diſtance from them; it muſt be denſer in ſmall than in large pores, denſer within the ſubſtance of bodies than at their ſurfaces, and denſer at their ſurfaces than at a diſtance from them. But no other property can account for the extraordinary quantity of this fluid contained within the ſubſtance of electrics *per ſe*, or for the common atmoſpheres of all excited and electrified bodies.

To account for the attraction of light bodies, and other electrical appearances, in air of the ſame denſity with the common atmoſphere, when glaſs (which is ſuppoſed to be impermeable to electricity) is interpoſed; it is conceived, that the addition or ſubſtraction of the electric fluid, by the action of the excited electric, on one ſide of the glaſs, occaſions (as in the experiment of the Leyden phial) a ſubſtraction or addition of the fluid on the oppoſite ſide. The ſtate of the fluid, therefore, on the oppoſite ſide being altered, all light bodies within the ſphere of its action muſt be affected, in the very ſame manner as if the effluvia of the excited electric had actually penetrated the glaſs, according to the opinion of all electricians before Dr. Franklin.

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The manner in which *clouds* acquire their pofitive or negative electricity is not determined, according to this, or any other theory, with fufficient certainty. Mr. Canton's conjecture is, that the air refembles the tourmalin, and, confequently, acquires its electricity by heating or cooling; but whether it gains or lofes, the electric fluid in either ftate muft be determined by experiment. Signior Beccaria's theory of the electricity of the clouds has been related at large.

This hypothefis of pofitive and negative electricity has been adopted, and, in fome meafure, rendered more fyftematical by Mr. Æpinus, in his elaborate treatife entitled, *Tentamen Theoriæ Electricitatis et Magnetifmi.*

He has extended the property of impermeability to air and all electrics as well as glafs, and defined it in a better manner; fuppofing impermeability to confift in the great difficulty with which electric fubftances admit the electric fluid into their pores, and the flownefs with which it moves in them. Moreover, in confequence of this impermeability of air to the electric fluid, he denies the reality of electric atmofpheres, and thinks, as was obferved before, that Dr. Franklin's theory will do much better without them.

He thinks that all the particles of matter muft repel one another: for that, otherwife (fince all fubftances have in them a certain quantity of the electric fluid, the particles of which repel one another, and are attracted by by all other matter) it could not happen, that bodies in their natural ſtate, with reſpect to electricity, ſhould neither attract nor repel one another.

He that reads the firſt chapter, as well as many other parts of his elaborate treatiſe above mentioned, may ſave himſelf a good deal of time and trouble by conſidering, that the reſult of many of his reaſonings and mathematical calculations cannot be depended upon; becauſe he ſuppoſes the repulſion or elaſticity of the electric fluid to be in proportion to its condenſation; which is not true, unleſs the particles repel one another in the ſimple reciprocal ratio of their diſtances, as Sir Iſaac Newton has demonſtrated, in the ſecond book of his Principia.

Mr. Wilcke, as well as Mr. Æpinus, adopts all the general principles of Dr. Franklin's theory of poſitive and negative electricity, but thinks that no experiments which have hitherto been made ſhow which of the electricities is poſitive and which negative. Suppoſing, however, what is called poſitive to be really ſo, and that ſmooth glaſs, for inſtance, rubbed upon ſulphur attracts the electric fluid from it, he would account for it upon the ſame principles whereby water ſtands in drops on rough ſurfaces, but is diffuſed on ſmooth ones. The electric fluid, he would ſuppoſe, is more ſtrongly attracted by the ſmooth ſurface of the glaſs, and therefore diffuſes itſelf over it, while it retreats from electrics of rougher ſurfaces *. But this explanation, I imagine, will give little ſatisfaction to ſceptical electricians.

MR. WILCKE acknowledges there is great difficult in accounting for the repulſive power of bodies electrified negatively, and thinks that it obliges us to ſuppoſe the mutual repulſion of all homogeneous matter. Mr. Waitz, he ſays, was of the ſame opinion. According to him, therefore, bodies which have too great a proportion either of the electric fluid, or of their proper conſtituent matter, muſt avoid one another. In the former caſe, by the repulſion of the electric fluid, in the latter, by the repulſion of the conſtituent parts of the bodies. Mr. Wilcke obſerves upon this ſubject, that the attraction of light bodies to negative electrics cannot be owing to the repulſive power of the electric fluid in the neighbouring air, driving them, or the electric matter in them to the place where there is a want of it; becauſe the velocity ought to decreaſe as it recedes from the impulſive power: whereas it is accelerated, as if it were attracted by the negative electric †.

BUT to this it may be replied, that a ſucceſſion of impulſes, though every ſubſequent one ſhould be weaker than the preceding, will produce an accelerated motion. Beſides, the nearer the light body is to the negative elec-

* Wilcke, p. 65.

† Ibid. p. 15, and Remarks on Franklin's Letters, p. 270.

tric,

ric, the nearer it is to the point where the equilibrium of the fluid is moſt deſtroyed; or the leſs force there is on the ſide of the electric to balance the force that drives the light body towards it, and therefore the impulſes themſelves muſt increaſe.

Mr. Wilcke, whoſe treatiſe on the two electricities is admirable, both for its materials and methodical arrangement of them, diſtinguiſhes three cauſes of excitation, viz. *warming*, *liquefaction*, and *friction*; and he adviſes, that we carefully diſtinguiſh between *ſpontaneous* and *communicated* electricity. By the former he means that which is the reſult of the appoſition, or mutual action of two bodies; in conſequence of which, one of them is left poſitively electric and the other negative. Whereas *communicated* electricity is that which is ſuperinduced upon a body, or part of a body, electric or non-electric, without its being previouſly heated, melted, or rubbed; or without any mutual action between it and any other body. This diſtinction is, in general, very obvious; but Mr. Wilcke defines it more accurately than it had been done before, and mentions ſeveral caſes in which they are often confounded.

Signior Beccaria admits the theory of poſitive and negative electricity, though he explains ſome electrical phenomena in a manner different from the other patrons of that ſyſtem.

He ſuppoſes that electrified bodies move to one another only in the act of giving and receiving the electric fluid *: this effect being produced by the electric matter making a vacuum in its paſſage, and the contiguous air afterwards collapſing, and thereby puſhing the bodies together †. This vacuum, he ſays, is very obſervable upon great exploſions of thunder, when animals have been ſtruck dead without being touched with the lightning; a vacuum being only ſuddenly made near them, and the air immediately ruſhing out of their lungs to fill it, whereby they are left flaccid and empty; whereas when perſons are properly killed by lightning, their lungs are found diſtended ‡.

In confirmation of this hypotheſis, he ſays, that leſs motion is given to bodies by electricity, as the air is excluded from them, and that in vacuo no motion at all can be given to them ‖. He alſo ſays, that no electric light is viſible in a barometer in which there is a perfect vacuum: whence he infers, that electric light is viſible only by means of ſome vibrations which it excites in the air §.

To account for the collection or diſſipation of electricity by points, he ſays that the electric fluid appears, from experiments, to move with the greateſt violence in the ſmalleſt bodies. All electrical appearances will, therefore, be moſt ſenſible at the points of bodies; and, conſequently, it will be ſooneſt diſſipated

* Lettere dell' elettriciſmo, p. 36. † Ibid. p. 41. ‡ Ibid. p. 42. ‖ Ibid. p. 48. § Ibid. p. 50.

there.

there. But this does not ſeem to touch the real difficulty.

Dr. Franklin, the author of this excellent theory of poſitive and negative electricity, with a truly philoſophical greatneſs of mind, to which few perſons have ever attained, always mentions it with the utmoſt diffidence. Every appearance, ſays he, which I have yet ſeen, in which glaſs and electricity are concerned, are, I think, explained with eaſe by this hypotheſis. Yet, perhaps, it may not be a true one, and I ſhall be obliged to him who affords me a better *.

It is no wonder, indeed, that this excellent philoſopher ſhould treat even his own hypotheſis with ſuch indifference, when he had ſo juſt a ſenſe of the nature, uſe, and importance of all hypotheſes. Nor is it, ſays he, of much importance to us, to know the manner in which nature executes her laws. It is enough if we know the laws themſelves. It is of real uſe to us to know that china left in the air, unſupported, will fall and break; but how it comes to fall, and why it breaks, are matters of ſpeculation. It is a pleaſure indeed to know them, but we can preſerve our china without it †.

The great merit of this writer as an electrician ſtands independent of all hypotheſes, upon the firm baſis of the diſcovery of many new and import facts, and, what is more,

* Franklin's Letters, p. 78. † Ibid. p. 59.

applied to the greateſt uſes. Suppoſing him, for inſtance, to have been miſtaken in his account how the clouds come to be poſſeſſed of electricity, muſt not all the world acknowledge themſelves indebted to him for the diſcovery of the ſameneſs of the electric fluid and the matter of lightning; and eſpecially for a certain method of preſerving their buildings and perſons from the fatal effects of thunder ſtorms.

SECTION

SECTION III.

OF THE THEORY OF TWO ELECTRIC FLUIDS.

CONVINCED, as the reader may have perceived that I am, of the ufefulnefs of various theories, as fuggefting a variety of experiments, which lead to the difcovery of new facts; he will excufe me, if I recall his attention to the old theory of vitreous and refinous electricity, as it was firft fuggefted by Mr. Du Fay, upon his difcovery of the different properties of excited glafs, and excited amber, fulphur, rofin, &c. and as it has been new modelled by Mr. Symmer. To fhow my abfolute impartiality, I fhall, notwithftanding the preference I have given to Dr. Franklin's theory, endeavour to reprefent this to as much advantage as poffible, and even to do it more juftice than has yet been done to it, even by Mr. Symmer himfelf; who, as I obferved before, has fallen into fome miftakes in his application of it. Indeed, hitherto very little pains has been taken with this theory, nor has it been extended to any great variety of phenomena.

LET us fuppofe then, that there are two electric fluids, which have a ftrong chymical affinity with each other, at the fame time that the

the particles of each are as ſtrongly repulſive of one another. Let us ſuppoſe theſe two fluids, in ſome meaſure, equally attracted by all bodies, and exiſting in intimate union in their pores, and while they continue in this union to exhibit no mark of their exiſtence. Let us ſuppoſe that the friction of any electric produces a ſeparation of theſe two fluids, cauſing (in the uſual method of electrifying) the vitreous electricity of the rubber to be conveyed to the conductor, and the reſinous electricity of the conductor to be conveyed to the rubber. The rubber will then have a double ſhare of the reſinous electricity, and the conductor a double ſhare of the vitreous; ſo that, upon this hypotheſis, no ſubſtance whatever can have a greater or leſs quantity of electric fluid at different times. The quality of it only can be changed.

THE two electric fluids, being thus ſeparated, will begin to ſhow their reſpective powers, and their eagerneſs to ruſh into reunion with one another. With whichſoever of theſe fluids a number of bodies are charged, they will repel one another, they will be attracted by all bodies which have a leſs ſhare of that particular fluid with which they are loaded, but will be much more ſtrongly attracted by bodies which are wholly deſtitute of it, and loaded with the other. In this caſe they will ruſh together with great violence.

UPON this theory, every electric ſpark conſiſts of both the fluids ruſhing contrary ways, and

and making a double current. When, for inſtance, I preſent my finger to a conductor loaded with vitreous electricity, I diſcharge it of part of the vitreous, and return as much of the reſinous, which is ſupplied to my body from the earth. Thus both the bodies are unelectrified, the balance of the two powers being perfectly reſtored.

When I preſent the Leyden phial to be charged, and, conſequently, connect the coating of one of its ſides with the rubber, and that of the other with the conductor, the reſinous electricity of that ſide which is connected with the conductor is tranſmitted to that which is connected with the rubber, which returns an equal quantity of its vitreous electricity; ſo that all the vitreous electricity is conveyed to one of the ſides, and all the reſinous to the other. Theſe two fluids, being thus ſeparated, attract one another very ſtrongly through the thin ſubſtance of the intervening glaſs, and ruſh together with great violence, whenever an opportunity is preſented, by means of proper conductors. Sometimes they will force a paſſage through the ſubſtance of the glaſs itſelf; and, in the mean time, their mutual attraction is ſtronger than any force that can be applied to draw away either of the fluids ſeparately.

Having ſtated the general principles of this hypotheſis of two fluids, I ſhall now enter into a brief compariſon of it with that of a ſingle fluid, as explained by the mode of poſitive and negative electricity; that we may ſee

ſee which of them will account for the ſame facts in the eaſier manner and more agreeable to the analogy of nature in other reſpects. For, allowing that no fact can be ſhown to be abſolutely inconſiſtent with either of them; yet, certainly, that will be judged preferable, which is attended with the leaſt difficulty in conceiving of its mode of operation.

IN the firſt place, the ſuppoſition itſelf, of two fluids, is not quite ſo eaſy as that of one, though it is far from being diſagreeable to the analogy of nature, which abounds with affinities, and in which we ſee innumerable caſes of ſubſtances formed, as it were to unite with and counteract one another. Here, likewiſe, agreeable to the theory of two electric fluids, while thoſe ſubſtances are in union, we ſee nothing of their ſeparate and peculiar powers, though they be ever ſo remarkable. What, for inſtance, do we ſee of the ſtriking properties of the *acid* and *alkali* while they are united in a neutral ſalt? What powers in nature are more formidable than the vitreolic acid, and phlogiſton, and what more innocent than common ſulphur, which is a compoſition of them both, and from which the action of fire ſeparates them.

THE two fluids being ſuppoſed, the double current from the rubber to the conductor and from the conductor to the rubber is an eaſy and neceſſary conſequence. For if, upon the common ſuppoſition, the action of the rubber puts a ſingle fluid into motion in one direction,

tion, we might expect that, if there were two fluids, which counteracted each other, they would, by the fame operation, be made to move in contrary directions. And a perfon who has been ufed to conceive that a fingle fluid may be made to move either way, viz. from the conductor to the rubber, or from the rubber to the conductor, at pleafure, according as a rough or a fmooth globe is ufed, can have much lefs objection to this part of the hypothefis.

Admitting then this different action of the rubber and the electric upon the two different fluids, the manner of conveying electric atmofpheres, or powers to bodies is the fame on this as on any other theory; and it is apprehended, that the phenomena of negative electricity are more eafily conceived by the help of a real fluid, than by no fluid at all. Indeed Dr. Franklin himfelf ingenuoufly acknowledges, that he was a long time puzzled to account for bodies that were negatively electrified repelling one another; whereas Mr. Du Fay, who obferved the fame fact, had no difficulty about it, fuppofing that he had difcovered another electricity, fimilar, with refpect to the properties of elafticity and repulfion, to the former.

By this double action of the rubber, the method of charging a plate of glafs is exceedingly eafy to conceive. Upon this hypothefis, all the vitreous electricity quits its union with the refinous on the fide communicating with the rubber, and is brought over

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to the ſide communicating with the conductor; which, by the ſame operation, had been made to part with its reſinous electricity in return.

All the vitreous electricity being thus brought to one ſide of the plate of glaſs, and all the reſinous to the other, the phenomena of the plate while ſtanding charged, or when diſcharged, are, perhaps, more free from all difficulty than upon any other hypotheſis. When one of the ſides of the glaſs is conceived to be loaded with one kind of electricity, and the other ſide with the other kind; the ſtrong affinity between them, whereby they attract each other with a force proportioned to their nearneſs, immediately ſupplies a ſatisfactory reaſon, why ſo little of either of the fluids can be drawn from one of the ſides without communicating as much to the other. Upon this ſuppoſition, that conſequence is perhaps more obvious than upon the ſuppoſition of one half of the glaſs being crowded with the electric matter, and the other half exhauſted. In the former caſe, every attempt to withdraw the fluid from one of the ſides is oppoſed by the more powerful attraction of the other fluid on the oppoſite ſide. On the other hypotheſis, it is only oppoſed by the attraction of the empty pores of the glaſs.

Lastly, the exploſion upon the diſcharge of the glaſs has as much the appearance of two fluids ruſhing into union, in two oppoſite directions, as of one fluid, proceeding only

only in one direction. The ſame may be ſaid of the appearance of every common electric ſpark, in which, upon this hypotheſis, there is always ſuppoſed to be two currents, one from the electric, or the electrified body, and the other to it.

I DO not ſay that the bur which is uſually ſeen on both ſides of a quire of paper pierced by an electric exploſion, and the current of air flowing from the points of all bodies electrified negatively as well as poſitively, are material objections to the doctrine of a ſingle fluid. I have even ſhown how they may be explained in a manner conſiſtent with it; but upon the ſuppoſition of two fluids, and two currents, the difficulty of accounting for theſe facts would hardly have occurred.

THE phenomena of diſcharging a plate of glaſs, upon the hypotheſis of two fluids, are indeed injudiciouſly explained by Mr. Symmer; who ſuppoſes that the two fluids do not always make the whole circuit of the intervening conductors, but enter them, more or leſs, from each ſide of the plate, according to the ſtrength of the charge. But upon this ſuppoſition, the fire of the ſmalleſt charge performs the whole circuit, as well as the fire of the greateſt, in order to reſtore the equilibrium of the two fluids on each ſide of the glaſs.

IT is almoſt needleſs to obſerve, that the influence of points is attended with exactly the ſame difficulty upon this theory as upon the other. It is equally eaſy, or equally difficult, to ſuppoſe one fluid to enter and go out at the point of an electrified conductor, at different times, as to ſuppoſe that, of two fluids, one goes out, and the other goes in, at the ſame time.

THAT bodies immerged in electric atmoſpheres muſt acquire the contrary electricity, is quite as eaſy to conceive upon this, as upon any other hypotheſis. For, in this caſe, ſuppoſe the electrified body to be poſſeſſed of the vitreous electricity, all the vitreous electricity of the body which is brought near it will be driven backwards, to the more diſtant parts, and all the reſinous electricity will be drawn forwards. And when the attraction between the two electricities, in theſe different bodies, is ſo great as to overcome the oppoſition to their union, occaſioned by the attraction of the bodies that contained them, the form of their ſurfaces, and the reſiſtance of the interpoſing medium, they will ruſh together; an electric ſpark will be viſible between them, and the electricity of both will appear to be diſcharged; the prevailing electricity of each being ſaturated with an equal quantity of the oppoſite kind, from the other body.

THIS hypotheſis will likewiſe eaſily account for the difficulty of charging a very thick plate of glaſs, and the impoſſibility of charging it beyond a certain thickneſs: for theſe fluids at a greater diſtance, will attract one another leſs forcibly; and at a certain ſtill greater diſtance will not attract at all.

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Having given the moſt favourable view that I can of this hypotheſis of two electric fluids, I ſhall, with the ſame fairneſs, make the beſt anſwer I am able to the principal objection that will probably be made to it.

If it be aſked, why the two fluids, meeting on the ſurface of the globe, or in the electric exploſion, do not unite, by means of their ſtrong affinity, and make no farther progreſs; it may be anſwered, that the attraction between all other bodies and the particles of both theſe fluids may be ſuppoſed to be, at leaſt, as ſtrong as the affinity between the fluids themſelves; ſo that the moment that any body is diſpoſſeſſed of one, it may recruit itſelf, to its uſual point of ſaturation, from the other.

Besides, in whatever manner it be that one of the electric fluids is diſlodged from any body (ſince, upon every theory, the two electricities are always produced at the ſame time) the oppoſite electricity will, by the ſame action, be diſlodged from the other ſubſtance. And (as upon the common theory) whatever it be that diſlodges the fluid from any ſubſtance, it will be ſufficient to prevent its return; conſequently, ſuppoſing both the ſubſtances neceſſarily to have a certain proportion of electric matter, each may be immediately ſupplied from that which was diſlodged from the other.

The rubber, therefore, at the time of excitation, gives its vitreous electricity to that part of the ſmooth glaſs againſt which it has been preſſed, and takes an equal quantity of the reſinous in return. The glaſs, being a non-conductor, does not allow this additional quantity of vitreous electricity to enter its ſubſtance. It is therefore diffuſed upon the ſurface, and, in the revolution of the globe, is carried to the prime conductor. There (as in the experiments begun by Mr. Canton, and proſecuted by Mr. Wilcke, &c.) it repels the vitreous, and violently attracts the reſinous electricity; and (the points of the conductor favouring the mutual tranſition) the vitreous, which abounds upon the globe, paſſes to the conductor; and the reſinous, which abounds on the neareſt parts of the conductor ruſhes upon the globe. There it mixes with, and ſaturates what remained of the vitreous electricity, on the part on which it flows, and thereby reduces it to the ſame ſtate in which it was before it was firſt excited. Every part of the ſurface of the globe performs the ſame office, firſt exchanging electricities with the rubber, and then with the conductor.

The ſolution of this difficulty will likewiſe ſolve that of the electric exploſion, in which there is a colliſion, as it were, of the two fluids, while yet they completely paſs one another. For ſtill each ſurface of the glaſs may be ſuppoſed to require its certain portion of electric matter, and therefore cannot part with one ſort without receiving an equal quantity of the other. It muſt be conſidered alſo, that the air, through which theſe fluids paſs, has already its natural quantity of electricity,

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ſo that being fully ſaturated, it can contain no more; and the two fluids only ruſh to the places from whence they had been forcibly diſlodged, and where the greater body of the oppoſite fluid waits to embrace them.

Mr. Symmer's hypotheſis of a double current differed in ſome reſpects from that of the Abbé Nollet. The Abbé, however, according to his uſual candour, ſpeaks of him with the higheſt reſpect; at the ſame time, he ſtill appears an advocate for his old favourite hypotheſis.

Mr. Cigna, who purſued the experiments above recited of Mr. Symmer, obſerves, with reſpect to his theory; that it is not contradicted by any phenomena that are yet known, and that it ſuits ſome of them in a peculiarly clear and elegant manner; particularly every thing relating to charging and diſcharging a plate of glaſs; all the experiments which ſeem to ſhow a mutual attraction between the two electricities, when they are kept aſunder; and that curious experiment above mentioned of Signior Beccaria, of diſcharging a plate of glaſs ſuſpended by a ſilken ſtring, without either touching or moving the plate. Yet, upon the whole, he declares in favour of Dr. Franklin's theory of poſitive and negative electricity, on account of its admirable ſimplicity, and becauſe philoſophers ought not to multiply cauſes without neceſſity.

Dr. Franklin's theory, he ſays, completely ſolves all the caſes of the two electricities deſtroying one another when they are

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mixed; but doth not ſo clearly account for their attracting, and counteracting one another when they are ſeparate. He concludes with ſaying, he doth not chuſe to ſay much on ſo very obſcure a queſtion, which has divided the opinions of very great men; and that any hypotheſis of the two electricities, which will account for the deſtruction of all the ſigns of electricity when they are united, and their mutual attraction when they are ſeparate, will equally ſuit all the phenomena that have been diſcovered.

I HAVE taken a little pains with this theory, becauſe I thought it had been, hitherto, too much overlooked, and that ſufficient juſtice had not been done to it, even by thoſe who propoſed it. For the future, I hope it will be ſeen to more advantage, and appear a little more reſpectable among its ſiſter hypotheſes; and then, *valeat quantum valere poteſt.* If any electrician will favour me with the communication of any other theory, not obviouſly contradicted by facts, I ſhall think myſelf obliged to him, and ſhall think I do a piece of real ſervice to the ſcience in the publication of it. If more perſons favour me with more different theories, I ſhould think my book, as far as theories are of any uſe, ſo much the more valuable.

PART

PART IV.

DESIDERATA IN THE SCIENCE OF ELECTRICITY, AND HINTS FOR THE FARTHER EXTENSION OF IT.

SECTION I.

GENERAL OBSERVATIONS ON THE PRESENT STATE OF ELECTRICITY.

THAT real progreſs has been made in electricity, has, I preſume, been ſufficiently demonſtrated in the courſe of the preceding hiſtory; that a great deal ſtill remains to be done, will, I think, be evident from this part of the work. Thoſe perſons who think that nothing has been done to any purpoſe in Natural Philoſophy, or that the advances have been made very ſlowly, ſince the time of Sir Iſaac Newton, need only read the preceding hiſtory, to be convinced, both that a great deal has been done, and that the progreſs in this kind of knowledge, inſtead of being ſlow, has been amazingly rapid. To quicken the ſpeed of philoſophers in purſuing this progreſs, and at the ſame time, in ſome

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meaſure, to facilitate it, is the intention of this treatiſe, and more eſpecially of this part of it. When a traveller imagines he is near his journey's end, he is little ſolicitous about making diſpatch, thinking that, without any haſte, the labour of the day will quickly be over; whereas, if he find that, whatever progreſs he may have made, he has a great deal ſtill to make, he continues, or quickens his ſpeed.

THE principal reaſon why many ingenious perſons have ſo ſoon got to their *ne plus ultra* in philoſophical diſcoveries, has evidently been their attachment to favourite theories; which they imagined both accounted for all the phenomena that had been obſerved, and would likewiſe account for all that ſhould be obſerved. Having therefore attained to the great object of a ſcience, and diſcovered the ultimate and moſt general principles of it, there was nothing more that was worth their notice; it being beneath men of genius to ſpend their time in diverſifying effects, when there were no new cauſes to be found. I hope that what has hitherto been ſaid concerning the nature and uſe of hypotheſes, and about the progreſs and preſent imperfect ſtate of thoſe which reſpect electricity, will convince thoſe electricians who may not yet have been convinced of it, that our buſineſs is ſtill chiefly with *facts*, and the *analogy of facts*; that far too few of theſe have been diſcovered to aſcertain a perfect general theory, and that all that the preſent hypotheſes can do for

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us muſt conſiſt in ſuggeſting farther experiments.

If we look back upon the hiſtory of electricity, and conſider the ſtate of facts and of hypotheſes at any particular period of time paſt, we ſhall ſee that there was always the ſame apparent reaſon for acquieſcing in what had been done, as at preſent. The theories of the firſt electricians, lame and imperfect as they were, were yet ſufficient to account for all the facts they were acquainted with; and as for other facts, they could have no idea or apprehenſion of them, and therefore could not be ſolicitous about them.

Mr. Boyle, no doubt, was as fully ſatiſfied with his ſimple hypotheſis of the unctuous effluvia, as Mr. Nollet with his theory of affluences and effluences; or the greateſt part of the preſent race of electricians with that of poſitive and negative electricity. Mr. Haukſbee, when he made his ſurpriſing diſcoveries concerning the properties of electric light, and many curious circumſtances concerning electric attraction and repulſion, might very naturally think that little more was to be done. Indeed, who could have thought otherwiſe, when the ſcience was actually at a ſtand for ſeveral years after him? All that the indefatigable Mr. Grey (who made the great diſcovery of the communication of electric powers to bodies not electric per ſe) imagined to remain undone, were mere chimeras and illuſions. Mr. Du Fay, who made the diſcovery of vitreous and reſinous electricity, had no idea of the electric ſhock; and the German philoſophers, who accidentally obſerved it, knew nothing of its moſt remarkable properties. Notwithſtanding a great number of treatiſes on the ſubject of electricity appeared preſently after this diſcovery, and ſome of them very ſyſtematical, comprehending, no doubt, what the authors of them thought to be the whole of the ſcience, yet none of them had the leaſt idea of the amazing diſcoveries of Dr. Franklin, relating either to the Leyden phial, or to the nature of lightning. And though numbers of Dr. Franklin's admirers thought that he had exhauſted the whole ſubject, he himſelf was far from thinking ſo; and the hiſtory of electricity, ſince the date of his capital diſcoveries, demonſtrates that his ſuſpicion was true.

It may be ſaid, that there is a *ne plus ultra* in every thing, and therefore in electricity. It is true: but what reaſon is there to think that we have arrrived at it. Mr. Grey might have uſed the ſame language above twenty years ago; but every body will now acknowledge, that it would have been above twenty years too ſoon: and yet, I think, it is evident, that Mr. Grey had really more reaſon to think he had arrived at the *ne plus ultra* of electricity, than we have to think that we are arrived at it. Time has brought to light a great number of *incomplete*, as well as complete experiments, and perhaps more of the former than of the latter; concerning all which, as he could have no knowledge, ſo he he could have no doubts; ſo that, though we know much more than he did; we, at the ſame time, know how much more is unknown better than he could. Hitherto the acquiſition of electrical knowledge has been like the acquiſition of riches: the more we poſſeſs, the more we wiſh to poſſeſs; and, I hope, the more indefatigable we ſhall be to acquire the poſſeſſion of it.

One thing extremely uſeful to the progreſs of farther diſcoveries, is to know what has really been done by others, and where the ſcience ſtands at preſent. For want of this knowledge, many a perſon has loſt his time upon experiments which he might have known had either failed or ſucceeded with others; and which it was, therefore, not worth his while to repeat. But the ſources of this kind of information are too much ſcattered, and too diſtant for moſt perſons to have acceſs to them. This was the firſt motive of the preſent undertaking, intended to exhibit a diſtinct view of all that has been done in electricity to the preſent time, and likewiſe the order and manner in which every thing has been done; that electricians, having a diſtinct idea of what the progreſs of electrical knowledge has been, might ſee more clearly what remains to be done, and what purſuits beſt promiſe to reward their labour.

Indeed it is almoſt impoſſible for any perſon to read the hiſtory of electricity without gathering many hints for new experiments. When he has the whole before him at one view,

view, he can better bring the diſtant parts together; and from the compariſon of them, new lights may ariſe. When he ſees what experiments have failed, and what have ſucceeded; what branches of the ſcience have been moſt attended to, and what things ſeem to have been overlooked; what has been diſcovered by accident, and what by theory; when he ſees both the true lights which directed ſome happy diſcoverers, and the falſe lights which miſled others, he will have the beſt preparation for purſuing his own inquiries.

To point out many of the *deſiderata* in the ſcience of electricity, I am ſenſible, will, for this reaſon, be ſuperfluous to many perſons, and probably to moſt who will have read thus far of this treatiſe: for ſufficient hints of them muſt have been ſuggeſted by the perſual of the hiſtory. But if I have been anticipated in this part of my work by ſome of my readers, it will not diſpleaſe them to find it; and to others, the contents of this chapter will be peculiarly uſeful.

If, indeed, I had conſulted my reputation as a writer, or a philoſopher, I ſhould not have attempted this chapter at all. For not only will many of the articles which I ſhall now put down as *deſiderata* in the ſcience be ſoon no longer ſo, and even young electricians be able to give ſatisfactory anſwers to ſome difficult queries I am going to propoſe; but many of them will probably appear idle, frivolous, or extravagant ones; and, in a more advanced

advanced ſtate of the ſcience, it will hardly be imagined why I put them down at all. But if this chapter be a means of haſtening ſo deſirable an event, and of accelerating the progreſs of electrical knowledge, I am very willing that it ſhould, ever after, ſtand as a monument of my preſent ignorance.

"THESE thoughts," to adopt the words of Dr. Franklin, with much more propriety than he himſelf firſt uſed them, "are many "of them crude and haſty; and if I were "merely ambitious of acquiring ſome repu- "tation in philoſophy, I ought to keep them "by me, till corrected and improved by time "and farther experience. But ſince even "ſhort hints and imperfect experiments, in "any new branch of ſcience, being commu- "nicated, have often times a good effect, in "exciting the attention of the ingenious to "the ſubject, and ſo become the occaſion of "more exact diſquiſitions, and more com- "plete diſcoveries; you are at liberty," ſays he, to Mr. Collinſon, "to communicate this "paper to whom you pleaſe, it being of "more importance that knowledge ſhould in- "creaſe, than that your friend ſhould be "thought an accurate philoſopher."

I WOULD not even propoſe to draw up the following *queries* upon the plan of thoſe of Sir Iſaac Newton, at the end of his treatiſe on Optics. Many of them are ſuch, that I have hardly the moſt diſtant expectation of their being verified; but the attempt to verify them may

may poſſibly lead to ſome other diſcoveries of more importance. They are ſuch random thoughts as led to the new experiments I have made; and not having any more leiſure to purſue them myſelf, I freely impart them to my reader, that he may make as much advantage of them as he can: being determined, upon taking leave of the ſubject, *to write myſelf fairly out*, as Mr. Addiſon ſays; or, as the Spaniſh writers ſay, *to leave nothing in my inkhorn.*

HAPPY would it be for ſcience, if all philoſophers who are engaged in the ſame purſuits, would make one common chapter of all their hints and queries: and greatly honoured ſhould I think myſelf if the preſent chapter in this treatiſe might be made uſe of for that purpoſe, and if, in future editions of the work, it ſhould be looked into as the common receptacle of the *preſent deſiderata* among the whole body of electricians, and of their imperfect hints for new diſcoveries. With pleaſure ſhould I ſee each of them diſtinguiſhed by the name of ſome generous and illuſtrious contributor. A few, the reader will find, have been added to my own, and are diſtinguiſhed in this manner.

MANY perſons can throw out hints, who either have not leiſure, or a proper apparatus for purſuing them: others have leiſure, and a proper apparatus for making experiments, but are content with amuſing them-

themſelves and their friends in diverſifying the old appearances, for want of hints and views for finding new ones. By this means, therefore, every man might make the beſt uſe of his abilities for the common good. Some might ſtrike out lights, and others purſue them; and philoſophers might not only enjoy the pleaſure of reflecting upon their own diſcoveries, but alſo upon the ſhare they had contributed to the diſcoveries of others.

SECTION II.

QUERIES AND HINTS CALCULATED TO PROMOTE FARTHER DISCOVERIES IN ELECTRICITY.

I.

QUERIES AND HINTS CONCERNING THE ELECTRIC FLUID.

WHAT is the proportion of the ſeveral colours in electric light, in different caſes, and in different appearances of it?

Is not the electric light a real vapour ignited, ſimilar to that of phoſphorus; and may not experiments be, hereafter, made, where we ſhall have the exploſion, the ſhock, and the other effects of electricity, without the light? Is the electric light ever viſible except in vacuo? In the open air the electric fluid makes itſelf a vacuum in order to its paſſage.

COLLECT the electric fluid, not from the general maſs of the earth but from bodies of particular kinds, and obſerve if it have any different properties, with reſpect to light, &c.

Is it exactly the ſame at ſea, as on land; below the ſurface of the earth, as above it, &c. &c. &c.?

Dr. Franklin obſerved, that iron was corroded by being expoſed to repeated electric ſparks. Muſt not this have been effected by ſome acid? What other marks are there of an acid in the electric matter? May not its phoſphoreal ſmell be reckoned one?

Is there only one electric fluid, or are there two? Or is there any electric fluid *ſui generis* at all, diſtinct from the ether of Sir Iſaac Newton? If there be, in what reſpect does it differ from the ether?

Are the particles which affect the organ of ſmelling, as well as the particles of light, parts of the proper electric fluid, or are they merely adventitious, being, ſome way or other, brought into action by electricity?

Does not ſome particular order of the particles, which Sir Iſaac Newton ſuppoſes to be continually flying from the ſurfaces of all bodies, conſtitute the electric fluid; as others, he imagined, conſtitute the air, and others the ether, &c.?

Is it probable that there is even any temporary, or growing addition to, or diminution of the whole ſtock of electricity?

Whence ariſes the elaſticity of the electric fluid, and according to what law do its particles repel one another? *Mr. Price.*

Is not the electric matter the ſame with, or at leaſt, hath it not ſome near relation to that luminous matter which forms the ſolar atmo-

atmoſphere, and produces the phenomenon called the *zodiacal light*; which is thrown off principally, and to the greateſt diſtance, from the equatorial parts of the ſun, in conſequence of his rotation on his axis, extending viſibly in the form of a luminous pyramid, as far as the orbit of the earth; and which, according to Monſieur De Mairan's ingenious, and at leaſt, plauſible hypotheſis, falling into the upper regions of our atmoſphere, is collected chiefly towards the polar parts of the earth, in conſequence of the diurnal revolution, where it forms the aurora borealis?

May not the ſun be the fountain of the electric fluid; and the zodiacal light, the tails of comets, the aurora borealis, lightning, and artificial electricity as it is various, and not very diſſimilar modifications*?

Did not the ſulphureous ſmell draw our attention towards the vitreolic acid, the peculiar hiſſing noiſe accompanying the electric blaſt, ſpontaneouſly iſſuing, for inſtance, from the pointed wire of a fully charged phial, appears rather to mimic the exploſive action of deflagrating nitre; and may, accordingly, without much violence, be ſuppoſed to ariſe from the nitrous aerial acid, violently commenſtruating with the phlogiſton, which it either meets with in the air, or which is conveyed to it by the electrified body. Or, were we to adopt the hypotheſis of two diſtinction electric

* Monthly Review, October 1767, p. 353.

fluids,

fluids, we might, by way of temperament, propofe as a query, whether the nitrous acid, &c. may not be the conftant concomitant of thofe explofive pencils of light, which are obferved to dart from the points of bodies replete with the vitreous electricity; while the filent and languid luminous fpecks (refembling the fmall tip of a lighted match) appearing on the extremities of bodies endued with a refinous electricity, may as probably indicate the accenfion of a fulphureous matter, and confequently the prefence of the vitreolic acid, the electric fpark of explofion, appearing on the approach of the two bodies, being confidered as the effect, at leaft, in part of the menftrual action of thefe two acids on each other, &c. *

II.

QUERIES AND HINTS CONCERNING ELECTRICS AND CONDUCTORS.

In what does the difference between electrics and conductors confift? In other words, what is it that makes fome bodies permeable to the electric fluid, and others impermeable to it?

Are the pores of electric bodies fmaller than thofe of conductors, and do they con-

* Monthly Review, December 1767, p. 458.

tain very much, or very little of the electric fluid?

What is it in the internal ſtructure of bodies that makes them break with a poliſh? Perhaps all ſolid electrics do ſo.

Has elaſticity any connection with electricity, ſome electrics being extremely elaſtic?

What is the reaſon why, in ſome of Mr. Haukſbee's experiments, the electric light was viſible through a conſiderable thickneſs of very opaque electrics, as roſin, ſulphur, pitch, &c. but not through the thinneſt metallic conductors?

What ſimilarity is there in the proceſſes of calcination, vegetation, animalization, and in ſome meaſure cryſtalization; ſince all bodies which have gone through any of thoſe proceſſes, and perhaps no others, are found to be electrics?

Are not both electrics and conductors more perfect in their kind in proportion to their ſpecific gravity?

Will not water conduct electricity the beſt in its ſtate of greateſt condenſation; and metals the leaſt in their greateſt expanſion, as ſhown by a pyrometer?

Compare the inviſible effluvia of water with the inviſible effluvia of a burning candle, and alſo thoſe proceeding from other bodies, with reſpect to their power of conducting electricity.

Observe what degree of heat will diſcharge any given degree of electricity, in order

der to find in what degree heat makes air a conductor.

III.

QUERIES AND HINTS CONCERNING EXCITATION.

WHAT is the difference, in the internal ſtructure of electrics, that makes ſome of them excitable by friction, and others by heating and cooling?

WHAT have friction, heating, cooling, and the ſeparation after cloſe contact in common to them all? How do any of them contribute to excitation? And in what manner is one, or the other kind of electricity produced by rubbers and electrics of different ſurfaces?

Is not Mr. Æpinus's experiments of preſſing two flat pieces of glaſs together, when one of them contracts a poſitive and the other a negative electricity, ſimilar to the experiments of Mr. Wilcke concerning the production of electricity by the liquefaction of various ſubſtances in others; when the ſubſtance which melts and contracts is in one ſtate, and that which contains it is in the oppoſite? And are not both theſe caſes ſimilar to the excitation of the tourmalin, &c. by heating and cooling? In this caſe may not the tourmalin and the air act upon one another and be in oppoſite ſtates?

Is not the circumſtance common to all theſe caſes, ſome affection of that ſpace near the ſurface of the bodies in which the refractive power lies? When bodies which have been preſſed together within that ſpace recede from one another, more ſurface, and conſequently more of that ſpace is made, doth not the electric fluid flow into it from that body which has the leaſt power of retaining it, and which it can permeate with the moſt eaſe; when not being able to enter the ſubſtance of the other, it reſts upon its ſurface?

Are not the particles of the electric and rubber thrown into a vibration in the act of excitation, which makes frequent recedings of the parts from one another, and thereby promotes the effect above mentioned?

What is the real effect of putting moiſture or amalgam upon the rubber? Do not thoſe ſubſtances increaſe the power of excitation, as conductors more diſtant from the ſmooth glaſs, in the gradation of electrics, than the ſurface of the leather? Or do they only make the rubber touch in more points, or alter the ſurface of the rubber?

Has that difference of ſurface on which colour depends any influence upon the power of excitation?

The tourmalin and a veſſel of charged glaſs hermetically ſealed are both excited by heating and cooling. What other properties have they in common?

IV.

QUERIES AND HINTS CONCERNING ELECTRIFICATION.

Does electrification increaſe the exhalation of vapours either from cold or from boiling water? If it do, is the increaſed exhalation the ſame in all ſtates of the atmoſphere?

Does not the electric matter paſs chiefly on the ſurface of bodies?

Is the action of electrified bodies upon one another more properly an attraction or a repulſion?

Would not continued electrification promote putrefaction?

In what manner is the mutual repulſion of two bodies electrified negatively performed? Is it by the attraction of the denſer electric fluid in the neighbourhood, by the quantity of it which may be ſuppoſed to be accumulated on the ſurfaces of ſuch bodies in the manner deſcribed p. 430, or to the mutual repulſion of the particles of matter of which the bodies conſiſt?

V.

QUERIES AND HINTS CONCERNING THE POWER OF CHARGING ELECTRICS.

WHAT is the real operation of conductors in coating electric ſubſtances?

WHAT is the *maximum* of charging a glaſs jar, with reſpect to the quantity of its ſurface, covered by the coating? It is evident that ſome jars will diſcharge themſelves, when only a ſmall part at the bottom of them is coated, and when the exploſion is very inconſiderable.

ENDEAVOUR to charge a plate of glaſs with the coating preſſed into actual contact with its ſurface, by means of heavy weights. Alſo endeavour to excite a plate of glaſs in the ſame manner. It is pretty certain that, in the uſual method of exciting and charging, the real ſubſtance of the glaſs is not touched; and though water be attracted by glaſs; it may only be to a certain diſtance from it.

VI.

QUERIES CONCERNING THE ELECTRICITY OF GLASS.

THROUGH what thickneſs of glaſs will an excited electric, of any given ſtrength, attract and

and repel light bodies? Is not the ſame thickneſs the limit of charging glaſs with the electric fluid?

Is not a plate of glaſs contracted in its dimenſions by charging, the two electricities ſtrongly compreſſing it, ſo as to increaſe its ſpecific gravity?

Is the tone of a glaſs veſſel, made in the form of a bell, the ſame when it is charged as when it is uncharged? Or would the ringing of it make it more liable to break in thoſe circumſtances?

Does the electric fluid with which glaſs is charged reſide in the pores of the glaſs, or only on its ſurface; or rather within the ſpace that is occupied by the power of refraction, i. e. a ſmall ſpace within, and likewiſe without the ſurface?

Is the refractive power of glaſs the ſame when it is charged or excited?

How does the different refractive power of glaſs, or its denſity (which is probably in the ſame proportion with its refractive power) affect its property of being excited or charged?

Is there not a conſiderable difference in glaſs when it is new made, and when it has been kept a month or two, both with reſpect to excitation and charging?

Let glaſs of every different compoſition be tried both with reſpect to excitation and charging. Would it not be found that differences with reſpect to metallic ingredients,

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hardneſs, annealing, continuance in fuſion, &c. would influence both the properties; and that, in ſeveral caſes, the ſame circumſtance that was favourable to one would be unfavourable to the other?

Glass has hitherto been ſuppoſed to be full of the electric fluid, and its impermeability has been accounted for upon the difficulty with which the electric fluid moves in its pores. But may we not ſuppoſe the ſubſtance of glaſs to be abſolutely impermeable to electricity, that no foreign electric matter ever ſo much as enters a ſingle pore of it, but lodges wholly on its ſurface; for inſtance, between the point of contact and the real ſurface, or within the limits of the refractive power; that is, a little way on both ſides the ſurface. This place is, I think, on many accounts, extremely convenient to diſpoſe of the electric matter, whether we make it to conſiſt of two fluids, or of one. Their being kept aſunder, if there be two, or its being prevented from getting through, if there be but one, will be much eaſier to conceive in this caſe, than upon the ſuppoſition that the electric fluid *can* enter and move in the ſubſtance of the glaſs, though it can only enter and move with difficulty, as Mr. Æpinus expreſſes it. For, let the motion be ever ſo difficult, one would think that this circumſtance could only make it move ſo much the ſlower, and that, give the electricity in the charged plate of glaſs time enough, and it would at length, without any exter-

external communication, perform the journey to the other ſide, whither it has ſo ſtrong a tendency to go.

MOREOVER, one would think, that, upon the hypotheſis of the admiſſion of the electric fluid within the pores of the glaſs, when the diſcharge of a phial was actually made through the ſubſtance of the glaſs, it might be in a ſilent manner, without breaking the glaſs; whereas when the ſurfaces of the glaſs are ſuppoſed to be violently preſſed, and the pores of it not in the leaſt entered by any particle of the fluid, or fluids, the impoſſibility of the electric charge getting through the glaſs is evident, as well as the neceſſity of its breaking the glaſs, if it do force a paſſage.

VII.

QUERIES AND HINTS CONCERNING THE EFFECT OF ELECTRICITY ON ANIMAL BODIES.

Is the fluid on which electricity depends, at all concerned in any of the functions of an animal body? In what manner is the pulſe of a perſon electrified quickened, and his perſpiration increaſed?

MAY not the increaſed perſpiration of an animal body be greater in a moiſt atmoſphere than in a dry one, there being then more conducting particles in the atmoſphere, to act and react upon the effluvia in the pores of the body;

body; on which the copious perſpiration does, probably, in a great meaſure, depend?

VIII.

QUERIES AND HINTS CONCERNING THE ELECTRICITY OF THE ATMOSPHERE.

In what manner do the clouds become poſſeſſed of electricity?

Does the wind in any meaſure contribute to it?

Is it effected by the gradual heating and cooling of the air? If ſo, whether is it the heating or the cooling that produces poſitive electricity? Which ever it be, the contrary will probably produce negative electricity. Let the experiment be made by an electrical kite. *Mr. Canton.*

As thunder generally happens in a ſultry ſtate of the air, when it ſeems repleniſhed with ſome ſulphureous vapours; may not the electric matter then in the clouds be generated by the fermentation of ſulphureous vapours with mineral or acid vapours in the air? *Mr. Price.*

Mr. Wilcke ſuppoſes the air to contract its electricity, in the ſame manner as ſulphur and other ſubſtances do, when they are heated and cooled in contact with various bodies. Thus the air, being heated or cooled in the neighbourhood of the earth, gives electricity to the earth, or takes it from it, and the elec-

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trified air, being conveyed upwards, by various means, communicates its electricity to the clouds *.

LET rain, ſnow, and hail be received in inſulated veſſels, in different ſtates of the atmoſphere, to obſerve whether they contain any electricity, and in what degree.

MAY not the void ſpace above the clouds be always occupied with an electricity oppoſite to that of the earth? And may not thunder, earthquakes, &c. be occaſioned by the ruſhing of the electric fluid between them, whenever the redundancy in either is exceſſive? Is not the aurora borealis, and other electrical meteors, which are remarkably bright and frequent before earthquakes, ſome evidence of this?

Is not the earth in a conſtant ſtate of moderate electrification, and is not this the cauſe of vegetation, exhalation, and other the moſt important proceſſes in nature? Theſe are promoted by increaſed electrification. And is it not probable that earthquakes, hurricanes, &c. as well as lightning, are the conſequence of a too powerful electricity in the earth?

SUPPOSING earthquakes to be cauſed by the diſcharge of a redundant electricity from the ſurface of the earth, might they not be prevented, in countries ſubject to them, by kites conſtantly flying very high, with wires in the ſtrings, ſo as to promote an eaſy communication between the earth and the upper regions of the atmoſphere?

* Remarks on Franklin's Letters, p. 302.

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SECTION III.

BRANCHES OF KNOWLEDGE PECULIARLY USEFUL TO AN ELECTRICIAN.

IN the hiſtorical part of this work I have ſhown what has been done on the ſubject of electricity, and under the preceding *deſiderata*, I have endeavoured to give ſome idea of what yet remains to be done, with a few hints concerning farther experiments. In the cloſe of this part, I would willingly do ſomething more towards enabling my reader to make farther advances in electrical inquiries. However, all that can be done in this way muſt, in its own nature, be more imperfect than even the account of the *deſiderata*: for it is evident, that he who is able to teach others to make diſcoveries might make them himſelf. Notwithſtanding this, it is poſſible that ſome general obſervations may be of uſe to this purpoſe; ſuch for inſtance as Lord Bacon makes, in his *Novum Organon*; a book which, though it contain few or no philoſophical diſcoveries itſelf, has contributed not a little to the diſcoveries contained in others. A few ſuch general obſervations, confined to the ſubject of electricity, I ſhall endeavour to ſuggeſt in this place.

IT is an obſervation which the progreſs of ſcience daily confirms, that all truths are not only conſiſtent, but alſo connected with one another. The obſervation has, with no ſmall appearance of juſtice, been extended even to the arts; there being no two of them ſo remote, but that ſome of the methods and proceſſes uſed in the one have ſome analogy to ſome that are uſed in the other. Hence the knowledge of one art or ſcience is ſubſervient to the knowledge of others; and no perſon can preſume that he is perfectly maſter of any one, till he has received all the aſſiſtance he can from, at leaſt, all its ſiſter arts or ſciences.

INDEED the very exiſtence of the various arts and ſciences is almoſt a demonſtration of their relation to each other. For it were highly unreaſonable to ſuppoſe, that the elements of any new art or ſcience were diſcovered by means independent of the ſtudy or practice of thoſe already known. As it is by eaſy tranſitions that we paſs from one part of any particular ſcience to another, ſo it is by tranſitions equally eaſy that mankind have paſſed from one diſtinct ſcience to another. Conſequently, to thoſe previouſly diſcovered arts and ſciences muſt we have recourſe, in order to underſtand the full evidence, on which the firſt principles of any new art or ſcience reſt.

ELECTRICITY is by no means an exception to this general rule. It has its ſiſter ſciences as well as others. In purſuance of them were its own principles firſt diſcovered; and the farther proſecution of electrical experiments has

has ſhown its connection with more ſciences than it was at firſt apprehended to have any relation to. Now the ſtudy of all theſe cannot but, reciprocally, contribute to perfect and extend the knowledge of electricity.

GILBERT, the firſt of modern electricians, was led to make his electrical experiments by their relation to thoſe of magnetiſm, into which he was profeſſedly inquiring. The ſtudy of chymiſtry ſeems to have led Mr. Boyle to attend to electricity, as well as to other occult qualities of particular bodies. Electric light was conſidered by all thoſe who firſt obſerved it as a ſpecies of phoſphorus; and with this view was Mr. Haukſbee conducted in all the experiments he made upon it.

THESE, and other diſcoveries in electricity, having been made thus indirectly, excited the attention of philoſophers to the ſubject, and induced them to ſit down to the ſtudy of it in a direct and profeſſed manner. Upon this it ſoon appeared, that electricity was no ſecondary, or occaſional, but a principal, and conſtant agent in the works of nature, even in ſome of its grandeſt ſcenes; and that its agency, far from being confined to bodies of a particular claſs, extended its influence to all without exception; that the mineral, vegetable, and animal world, with the human frame in a particular manner, were all ſubject to its power; and that electrical experiments and principles enter into the moſt intereſting arts and ſciences which have them for their object.

object. We alſo ſee every day, that electricity is extending itſelf ſtill more into the ſubjects of other ſciences, both by means of the analogy of their operations, and alſo by their reciprocal influences.

On theſe accounts, to be an electrician at preſent, requires a much more extenſive fund of various knowledge than it did but ten years ago; and a man muſt have a very comprehenſive knowledge of nature in all its known operations, before he can reaſonably expect to make any farther diſcoveries. For it can only be by applying electricity to various parts of nature, and by combining its operations with other operations, both of nature and art, that any thing new can be found out. Almoſt all that can be done by the common electrical machines, and the uſual apparatus of them, has been done already; ſo that we muſt look farther in queſt of new diſcoveries. I hope, therefore, that I ſhall be excuſed, if I endeavour to give a hint of that kind of knowledge which, I apprehend, may be peculiarly ſubſervient to improvements in electricity, and furniſh views and materials for new experiments.

Natural philosophy cannot but be of the greateſt uſe for this purpoſe; but, of all its branches, none promiſes to be of more uſe to the electrician than chymistry. Here ſeems to be the great field for the extenſion of electrical knowledge: for chymiſtry and electricity are both converſant about the latent and leſs obvious properties of bodies; and

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yet their relation to each other has been but little confidered, and their operations hardly ever combined; few of our modern electricians having been either fpeculative or practical chymifts.

AMONG other branches of Natural Philofophy, let the doctrine of LIGHT AND COLOURS be alfo particularly attended to. It was this that Newton thought would be the key to other, at prefent, occult properties of bodies.

LET particular attention be alfo given to every thing that the imperfect ftate of Natural Philofophy furnifhes refpecting the ATMOSPHERE, its compofition and affections. The phenomena of lightning fhow the connection of this fubject with electricity; and, probably, electricity may be our key to a much more extenfive knowledge of meteorology than we are yet poffeffed of.

THE fhock of the Leyden phial, the difcovery of the famenefs of lightning and electricity, together with the cure of feveral difeafes by electrical operations, are fufficient to convince us of the fingular importance of the ftudy of ANATOMY, and every thing relating to the animal œconomy to an electrician. And had phyficians more generally attended to electricity, as an article of the materia medica, many more important and ufeful difcoveries might, no doubt, have been made. Enow, however, have been made to excite us to farther inquiries.

MR.

MR. ÆPINUS has lately given us an excellent ſpecimen of what uſe MATHEMATICS, and eſpecially algebraical calculations, may be to an electrician; and their uſe will probably, in time, be found ſtill more extenſive.

As electricity has much to expect from ſeveral branches of Natural Philoſophy, ſo it will be ready, in its turn, to lend its aſſiſtance to them. It already ſupplies arguments and proofs of ſome principles in Natural Philoſophy, which ſtrengthen thoſe that are drawn from other quarters. By electricity, as well as by the principles of light and colours, we can demonſtrate, that it requires a conſiderable force to bring bodies which are contiguous to one another, and even lie upon one another, into actual contact; and the moiſture of the air may perhaps be ſhown to more exactneſs by Mr. Canton's electrical balls than by any other hygrometer whatever. But I do not mean to purſue this ſubject, and only mention theſe caſes by way of example.

UPON the ſubject of the proper furniture for an electrician, I think it may be juſtly added, that a knowledge of MECHANICS will be uſeful to him; by which I mean, upon this occaſion, not only the theory, but in ſome meaſure the practice too. For without ſome mechanical knowledge of his own his electrical machinery will be very often out of order, and but ill anſwer his purpoſe.

If, indeed, a perſon mean nothing more than to amuſe himſelf and his friends with the experiments that have been made by others (and this is a method of amuſement which I am far from diſcouraging) the machines he may purchaſe, ready conſtructed to his hands, will anſwer his purpoſe very well; and the directions which are uſually given along with the machines will enable him to perform the common experiments with tolerable certainty: and if any damage ſhould happen to his apparatus, a mathematical inſtrument-maker (if he happen to live in or near a large town) can readily repair it for him. But if a man propoſe to ſtudy the ſubject of electricity as a philoſopher, with a view to extend the knowledge of it, the aſſiſtance of others will not be ſufficient for him.

The common electrical machines, and the uſual electrical apparatus, will enable a perſon to do little more than exhibit the common experiments. If he propoſe to go farther, he muſt diverſify his apparatus; he muſt often alter the conſtruction of his machines, and will find that common workmen cannot execute any thing out of their uſual way, without more than general directions. Beſides, unleſs a perſon be fortunately ſituated, workmen of every kind cannot always be at hand, to do every little thing he may want in the mechanical way, whenever he may happen to get a hint of a new experiment that requires it.

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An electrician, therefore, ought never to be without the common tools of a *cabinet-maker*, *clock*, and *watch-maker*, at least, and know, in some measure, how to use them. With respect to glass, he ought, by all means, to learn the use of a *blow-pipe*, the method of drawing out and bending glass tubes, and performing, with some degree of dexterity, other operations upon glass, which he will want to use in a great variety of forms. An electrician, thus furnished, will be able, upon any occasion, to serve himself: and the slowness and blunders of mechanics do but ill suit with the ardour of persons engaged in philosophical inquiries.

It were much to be wished, that philosophers would attend more than they do to the construction of their own machines. We might then expect to see some real and capital improvements in them; whereas little can be expected from mere mathematical instrument makers; who are seldom men of any science, and whose sole aim is to make their goods elegant and portable.

Formerly, indeed, philosophers were obliged to construct their own machines. Mr. Boyle, Mr. Hauksbee, and Dr. Desaguliers would have done nothing by giving tradesmen orders for what they wanted. There were no such things to had. Necessity therefore drove them to the study and practice of mechanics, and from thei contrivances are derived almoſt all the philoſophical inſtruments which are now in uſe.

EVERY original genius, like them, muſt, in this reſpect, follow their ſteps. He will extend his views beyond the power of the preſent machinery, which can only be adapted to the preſent ſtate of ſcience. And, I think, one principal reaſon of the imperfect ſtate of ſeveral branches of electrical knowledge with us, may be evidently traced to ſome general imperfections in the ſtructure of all our common machines in England; which render ſeveral kinds of experiments very difficult, or almoſt impoſſible to be made; as may be ſhown in the next part of the work, in which I ſhall treat at large of the conſtruction of machines, and give the beſt directions I am able for uſing them.

LASTLY, if an electrician intend that the public ſhould be benefited by his labours, he ſhould, by all means, qualify himſelf to draw according to the rules of PERSPECTIVE; without which he will often be unable to give an adequate idea of his experiments to others. There is ſo much beauty in the rules of this ingenious art, and ſo much pleaſure in the application of them, that I cannot help wondering, that all gentlemen of a liberal education do not take the ſmall degree of pains, that is neceſſary to make them-

themſelves maſters of it. All the mechanical methods of drawing, eſpecially where a great number of right lines are uſed, as in drawing machines, &c. are exceedingly imperfect, and inſufficient. They admit not of half the variety of perſpective drawings. They can hardly ever be near ſo correct; beſides that, I know by experience, they take up much more time, and the operation is exceedingly ſlaviſh and troubleſome.

PART V.

OF THE CONSTRUCTION OF ELECTRICAL MACHINES, AND THE PRINCIPAL PARTS OF AN ELECTRICAL APPARATUS.

SECTION I.

GENERAL OBSERVATION ON THE CONSTRUCTION OF AN ELECTRICAL APPARATUS.

IMPROVEMENTS in electrical machines have, as might well be expected, kept pace with improvements in the ſcience of electricity. While nothing more than electrical attraction and repulſion were known, nothing that we ſhould now call an *electrical apparatus* was neceſſary. Every thing that was known might be exhibited by means of a piece of amber, ſealing-wax, or glaſs; which the philoſopher rubbed againſt his coat, and preſented to bits of paper, feathers, and other light bodies that came in his way, and coſt him nothing.

To give a greater degree of friction to electric ſubſtances Otto Guericke and Mr. Haukſbee

Haukſbee contrived to whirl ſulphur and glaſs in a ſpherical form; but their limited knowledge of electricity did not ſuggeſt, or require the more complex ſtructure of a modern electrical machine: Mr. Haukſbee's contrivances, indeed, were excellent, and the apparatus for many of his experiments well adapted to the purpoſes for which they were intended.

When no farther uſe could be made of globes, philoſophers had recourſe to the eaſier and cheaper apparatus of glaſs tubes, and ſticks of ſulphur or ſealing-wax; and the firſt conductors they made uſe of were nothing more than hempen cords ſupported by ſilken lines. To theſe, bars of metal were ſoon ſubſtituted. After that, recourſe was again had to the globe, as much more convenient to give an uniform ſupply of electric matter to theſe inſulated conductors; and, in due time, a rubber was uſed to ſupply the place of a human hand.

The diſcovery of the Leyden phial occaſioned ſtill more additions to our electrical apparatus; and the more modern diſcoveries of Dr. Franklin and others have likewiſe made proportionable additions highly requiſite. No philoſopher, for inſtance, can now be ſatiſfied, if he be not able to ſupply a conductor from the clouds, as well as from the friction of his glaſs globes or tubes. But having already marked the progreſs of improvements in electrical machines, as well as in electrical ſcience, I ſhall content myſelf with this brief recapitulation, and proceed to deſcribe what experience (in many caſes dear bought) has taught me to think the beſt method of conſtructing machines, and to lay down the beſt rules for conducting electrical operations.

NOTWITHSTANDING globes or cylinders are now of the moſt extenſive uſe in electrical experiments, GLASS TUBES are, nevertheleſs, moſt convenient for ſeveral purpoſes, and no electrician ought to be without them. They ſhould be made as long as a perſon can well draw through his hand at one ſtroke, which is about three feet, or ſomething more; and as wide as he can conveniently graſp. The thickneſs of the metal is not material, perhaps the thinner they are, the better, if they will bear ſufficient friction; which, however, needs only to be very gentle, when the tube is in good order. It is moſt convenient to have the tube cloſed at one end: for, beſides that the electric matter is thereby retained beſt on its ſurface, the air may more eaſily be drawn out of it, or condenſed in it, by means of a braſs cap fitted to the open end. A tube thus furniſhed is requiſite for various experiments. [*a*. Pl. II.]

THE beſt rubber that has yet been found for a ſmooth glaſs tube is the rough ſide of black oiled ſilk, eſpecially when a little amalgam of mercury and any metal, is put upon it.

AN electrician ſhould be furniſhed with rough glaſs tubes, i. e. tubes with their poliſh taken

taken off, as well as with ſmooth ones; but a cylinder of baked wood will do nearly as well. The beſt rubber for a rough glaſs tube, or a cylinder of baked wood, as well as for a ſtick of ſulphur or ſealing-wax, is ſoft new flannel; or rather ſkins, ſuch as hare ſkins, or cat ſkins, tanned with the hair on, being ſmoother, and having a more exquiſite poliſh.

Electricians are not quite agreed whether the preference is, upon the whole, to be given to globes or cylinders. In favour of cylinders it is ſaid, that more of their ſurface may be touched by the rubber. On the other hand, in favour of globes, it is ſaid, that they can more eaſily be blown true, ſo as to preſs the rubber equally; they may alſo be made larger in diameter, and by this means, the axis (if they have any) may be farther from the excited ſurface: for when the *axes* are near the ſurface, the electric fire will ſeem to ſtrike them, ſo that they will ſometimes appear luminous in the dark, and if they be inſulated, the extremities of the axes will give ſparks; which is certainly a diminution of the electric fire at the conductor.

For this reaſon, I would adviſe, that all *axes* be avoided as much as poſſible, having found by experience, that they are in no caſe whatever neceſſary, the largeſt globes being whirled horizontally, with the greateſt eaſe, and in every reſpect to more advantage, with one neck than two. This method of fitting up up globes alſo makes electrical machines much leſs complex, expenſive, and troubleſome.

LET every globe intended to be thus fitted up have its neck incloſed in a pretty deep braſs cap, ending in a dilated brim, of about half an inch broad, if the globe be a large one. To this neck let there be fitted a ſhort iron axis, and on that a PULLEY; and let a ſpace of about three quarters of an inch of the axis be left between the pulley and the cap In this place the axis is to be ſupported by a ſtrong BRASS ARM [*c.* Pl. VII.] proceeding from the pillar into which the extremity of the axis is put, and in which it turns. This braſs arm may be made to receive globes of any ſize whatever, room being left in it for pullies of any ſize that may be wanted for them.

IN this manner globes may be fixed much more truly than they can with two necks, and they are mounted with much more eaſe, and leſs expence. The weight of large globes is no objection to this method. The largeſt need not to weigh above eight or ten pounds, and theſe have been found to turn with great eaſe in this manner. The rubber, if it be placed under the globe, will contribute to ſupport the weight of it.

LET there be a hole made in the braſs cap above mentioned, in order to preſerve a communication between the external air, and the air within the globe: for if the air within the globe be either rarer or denſer than that without, the excitation is found to be leſſened in propor-

proportion; and, judging from experience, nothing is to be apprehended from any moiſture which might be ſuppoſed to inſinuate itſelf into the globe by ſuch a communication. A difference, however, might be found in a damp ſituation.

It will be found convenient to have the axis project about an inch beyond the pillar in which it turns, [as at *d*. Pl. VII.] that a handle may be fitted to it, and that it may thereby be turned without a wheel, for the greater variety of experiments.

If an axis be uſed, let both the extremities of it be carefully turned in a lathe; otherwiſe it will not turn without a very diſagreeable rattling; and let the part within the globe be made round, and ſmooth, or covered with ſome electric ſubſtance, to prevent its taking off much of the electric virtue of the globe.

One of the pillars, in which theſe globes or cylinders with two axes are turned, ſhould be moveable; for then a globe or cylinder of any ſize may be uſed, and they ſhould be made high enough, and have holes at ſmall diſtances quite to the top, to take two globes upon occaſion, one above the other. [Plate VII.]

It has not yet been determined by electricians what kind of glaſs is the fitteſt for electrical purpoſes, but the beſt flint is commonly uſed. I have not made ſo many experiments, as I could wiſh, to aſcertain this circumſtance; as they are both very uncertain

tain and expenſive; but I have ſome reaſon to think that common bottle metal is fitteſt for the purpoſe of excitation; at leaſt, the beſt globe I have yet ſeen is one that I have of that metal. Its virtue is certainly exceeding great, and I attribute it in part to the great hardneſs of the metal, and in part to its exquiſite poliſh. The blowing of any thing ſpherical in this metal, and eſpecially the making large globes ſmooth is very precarious; and they can hardly be made with two necks.

THE globe above mentioned is about ten inches in diameter, but nothing has been determined about the beſt ſize. I have uſed almoſt every ſize, from three inches to near eighteen in diameter, without knowing what advice to give. Perhaps *ceteris paribus*, twelve or thirteen inches may be, upon the whole, as convenient as any; but much larger, if they could be whirled with the ſame eaſe, would probably do better.

IF a perſon chuſe to have no aſſiſtant, but would turn the globe, and manage the apparatus himſelf, (which is, on many accounts, very deſirable) it will be moſt convenient to have the axis of the WHEEL level with the table at which he ſits. But if he chuſe rather to ſtand all the time he is making his experiments, it ſhould be raiſed proportionably higher. It will, perhaps, be moſt convenient to make the diameter of the wheel about eighteen inches; and the diameter of the pullies ſhould be ſuch as will give them, at leaſt, four or five revolutions for one of the wheel.

wheel. For the globe ſhould generally revolve at leaſt four or five times in a ſecond, which is much ſwifter than it can well be turned without a wheel. But if a globe be very large, a wheel is leſs neceſſary.

The wheel ſhould be made moveable with reſpect to the frame in which the globes are hung, or the frame ſhould be moveable with reſpect to the wheel, to ſuit the alterations which the weather will make in the length of the *ſtring*, particularly if it be made of hemp; but worſted makes an excellent ſtring, and is not ſo apt to alter with the weather, and a leathern ſtrap is perhaps better than either. If the diſtance between the wheel and the pulley cannot be altered, the operator muſt occaſionally moiſten his hempen ſtring, in order to make it tighter, which is, on many accounts, very inconvenient. Several grooves in the ſame wheel are very uſeful, and almoſt neceſſary, if more than one globe be uſed at the ſame time. If a flat leathern ſtrap be not uſed they ſhould be cut ſharp at the bottom; as ſhould alſo the grooves in the pulleys, that the ſtring may lay faſter hold of them, and that ſtrings of different ſizes may be uſed.

The beſt RUBBERS for globes or cylinders are made of red baſil ſkins, particularly the neck part of them, where the grain is more open, and the ſurface rather rough. That the rubber may preſs the globe equally, it ſhould be put upon a plate of metal bent to the ſhape of the globe, and be ſtuffed with any thing that is pretty ſoft. Bran is good; and if the ſtuffing be a conductor, as flax, it will be better than if it be a non-conductor, as hair, or wool. It ſhould reſt upon a ſpring, to favour any inequality there may be in the form of the globe or cylinder. The beſt poſition of the rubber, for a variety of purpoſes, is an horizontal one, but it ſhould be capable of being placed in every variety of horizontal poſition; and the ſpring which ſupports the rubber ſhould be made to preſs more or leſs at pleaſure. The rubber ſhould be made nearly as large one way as the other. If it be made very narrow, ſome parts of the globe will paſs it without a ſufficient friction. To remedy that inconvenience, the hand (if it be dry) may be held to the globe, juſt before the rubber, to add to its breadth; but that poſture is very inconvenient.

It is adviſable that there be no ſharp edges or angles about the rubber, for that would make the *inſulation* of it (which is a matter of great conſequence) ineffectual. By the inſulation of the rubber every electrical experiment may be performed with the twofold variety of poſitive and negative, and a conductor be made to give or take fire at pleaſure. This inſulation is beſt made by a plate of glaſs, five or ſix inches in diameter, [*g*. Pl. VII.] interpoſed between the metallic part of the rubber and the ſteel ſpring that ſupports it. When poſitive electricity is intended to be produced, a chain [*n*. Pl. VII.] muſt connect the rubber with the floor; but when negative elec-

electricity is wanted, the chain muſt be removed, and hung upon the common conductor, while another prime conductor muſt be connected with the rubber ; which will therefore be electrified negatively.

THE beſt method of collecting the electric fire from the globe ſeems to be by three or four pointed wires, [*m*. Pl. VII.] two or three inches long, hanging lightly upon the globe ; and neither ſo light as to be thrown off the globe by electrical repulſion (which would occaſion a loſs of the electric matter) nor ſo heavy as to prevent their ſeparating to a proper diſtance, and being drawn backwards or forwards, as the moſt effectual diſcharge of the fire, accumulated on different parts of the globe, may require. For this purpoſe they are beſt ſuſpended on an open metallic ring. Needles with fine points do admirably well.

IT is requiſite, for a variety of uſes, that the PRIME CONDUCTOR be fixed very ſteady. It ought not, therefore, to hang in ſilken ſtrings, but have a ſolid ſupport. In a dry ſituation baked wood anſwers very well ; but a hollow pillar of glaſs lined with ſealing-wax is better, as it doth not require ſo much attention.

FOR common purpoſes a ſmall conductor is moſt convenient, but where a ſtrong ſpark is wanted, it is proper to have a large conductor at hand, which may occaſionally be placed in contact with the ſmaller, and be removed from it at pleaſure. But whatever be the ſize of a prime

prime conductor, the extremity of it, or that part which is moſt remote from the globe, ſhould be made much larger and rounder than the reſt: [*k*. Pl. VII.] for the effort of the electric matter to fly off is always the greateſt at the greateſt diſtance from the globe. But for the ſame reaſon, if a *long* ſpark be wanted, the large conductor ſhould terminate in a ſmaller knob, or an obtuſe edge, at which the ſparks ſhould be ſolicited. Experience only can inſtruct a perſon what ſize of a knob, or what thickneſs of an edge is the beſt for the purpoſe. In this reſpect, the effects are often very different in the ſame apparatus, when the difference in the circumſtances is imperceptible.

As the electrician will have frequent occaſion to inſulate various bodies, I would adviſe that he make all the ſtands and ſtools which he uſes for that purpoſe of glaſs tubes lined with ſealing-wax, though in a dry ſituation; baked wood, eſpecially when covered with a ſlight varniſh, will do very well.

THE electrician, having thus conſtructed his machine, will want METALLIC RODS, [*s*. Pl. II.] to take ſparks from his conductor for various uſes. Theſe ſhould have knobs, larger or ſmaller in proportion to the curvature of the conductor. If the knob be too ſmall, it will not diſcharge the conductor at once, but by degrees, and with a leſs ſenſible effect; whereas the ſpark between broad ſurfaces is thick and ſtrong.

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The more formidable part of an electrical apparatus consists in the COATED GLASS, that is used for the Leyden experiment. The form of the plate is immaterial with respect to the shock; and, for different experiments, both plates of glass, and jars, of various forms and sizes, must be used. For common uses, the most commodious form is that of a jar, as wide as a person can conveniently hold by grasping, and as tall as it will stand without any danger of falling; perhaps about three inches and a half in diameter, and eight inches in height. The mouth should be pretty open, that it may be the more conveniently coated on the inside, as well as the outside, with tinfoil: but it will generally be most convenient to have the mouth narrower than the belly; for then it may be more easily kept clean and dry, and the cork, when one is wanted, will be easier to manage. But no electrician would chuse to be without a great number of jars of various sizes and forms. A considerable variety may be seen in plate II. fig, *c*, *d*, *e*, *f*, *g*, *h*, *i*, *j*, *k*. The form of a coated plate of glass is represented at *b*, in the same plate.

The method of coating is much preferable to that of putting water or brass shavings into the jars, which both makes them very heavy, and likewise incapable of being inverted, which is requisite in many experiments. Brass dust, however, or leaden shot is very convenient for small phials. These serve very well where it is necessary to remove the coating as soon as the jar is charged, but, for this purpoſe, quickſilver will generally anſwer the beſt. The tinfoil may be put on either with paſte, gum water, or bees wax. To coat the inſides of veſſels, which have narrow mouths, moiſten the inſide with gum water, and then pour ſome braſs duſt upon it. Enough will ſtick on to make an exceedingly good coating; and if nothing very hard rub againſt it, it will not eaſily come off. This braſs duſt, which is extremely uſeful in a great variety of electrical experiments, may be had at the pin-makers.

IN the conſtruction of an ELECTRICAL BATTERY I would not, in general, recommend very large jars. A number of ſmaller are preferable on ſeveral accounts. If one of theſe break by an exploſion, or be cracked by any accident, the loſs is leſs conſiderable; beſides, by means of narrow jars, a greater force (that is a greater quantity of coated ſurface) may be contained in leſs room; and, as narrow jars may be made thinner, they will be capable of being charged higher in proportion to their ſurface than large jars, which muſt neceſſarily be made thick. The largeſt jars that the glaſs-men can conveniently make are about ſeventeen inches in height; and they ſhould not be more than three in diameter, and of the ſame width throughout. Thus they may be eaſily coated both within and without, and a box of a moderate ſize will contain a prodigious force: for the jars being coated within two inches of the top,

they

they will contain a ſquare foot of coated glaſs a piece.

The firſt battery that I conſtructed for my own uſe, conſiſted of forty-one jars of this ſize; but a great number of them burſting by ſpontaneous diſcharges, I conſtructed another, which I much prefer to it, and of which a drawing is given Plate III. It conſiſts of ſixty-four jars, each ten inches long, and two inches and a half in diameter, coated within an inch and a half of the top. The coated part of each is half a ſquare foot; ſo that the whole battery contains thirty-two ſquare feet. The wire of each jar has a piece of very ſmall wire twiſted about the lower end of it, to touch the inſide coating in ſeveral places; and it is put through a pretty large piece of cork, within the jar, to prevent any part of it touching the ſide, which would tend to promote a ſpontaneous diſcharge. Each wire is turned round, ſo as to make a hole at the upper end; and through theſe holes a pretty thick braſs rod with knobs is put, one rod ſerving for one row of the jars.

The communication between theſe rods is made by laying over them all a thick chain, which is not drawn in the plate, leſt the figure ſhould appear too confuſed. If I chuſe to uſe only part of the battery, I lay the chain over as many rods as I want rows of jars. The bottom of the box, in which all the jars ſtand, is covered with a plate of tin, and a *bent wire*, touching this plate, is put through the box, and appears on the outfide, as in the plate. To this wire is faftened whatever is intended to communicate with the outfide of the battery, as the piece of fmall wire in the figure, and the difcharge is made by bringing the brafs knob to any of the knobs of the battery.

THIS is the battery which I have generally ufed in the experiments related in the laft part of this work; though, when I have wanted a very great force, I have joined both the batteries, and even feveral large jars to them. And it will perhaps be allowed to be fome evidence of the goodnefs of this conftruction, that after ufing it fo much, I fee no no reafon to wifh the leaft alteration in any part of it. Were I to conftruct another battery, I fhould take jars of the very fame fize, and difpofe of them in the very fame manner.

To difcover the kind and degree of electricity, many forms of ELECTROMETERS have been thought of, as the reader may have perceived in the courfe of the hiftory; but this bufinefs is ftill imperfect. Mr. Canton's balls are of excellent ufe both to difcover fmall degrees of electricity, to obferve the changes of it from pofitve to negative, and *vice verfa*; and to eftimate the force of a fhock before the difcharge, fo that the operator fhall always be able to tell, very nearly, how high he has charged his jars, and what the explofion will be whenever he chufes to make it.

MR.

MR. CANTON'S BALLS (repreſented on a glaſs ſtanding on the ſtool [*c* Plate II.] are only two pieces of cork, or pith of elder, nicely turned in a lathe, to about the ſize of a ſmall pea, and ſuſpended on fine linen threads. It is convenient to have theſe in ſmall boxes for the pocket; the box being the full length of the ſtrings, that they may lie there without being bent.

MR. KINNERSLEY'S electrometer, deſcribed vol. i. p. 255, is uſeful to aſcertain how great ſhocks have been, and for many curious experiments in electricity. A drawing is given of it [*r*, Plate II.] but the glaſs tube is repreſented as much ſhorter than it was made by Mr. Kinnerſley. I think it in general more convenient; as the bore of the ſmall tube may eaſily be proportioned to it. But if a perſon get one long tube, of the ſame ſize throughout, it may be cut into different lengths, and the ſame braſs caps will fit any part of it.

AT the top of the ſtand of baked wood which ſupports Mr. Kinnerſley's electrometer, I have fixed another, contrived by Mr. Lane, to give a number of ſhocks, all of preciſely the ſame degree of ſtrength. It conſiſts of a braſs rod furniſhed with a knob, which, by means of a fine ſcrew, may be fixed at any diſtance from the prime conductor, or any other fixed body communicating with the inſide of a jar or battery. In conſequence of this, the jar or battery, with which it is connected, can be charged no higher than the diſtance at which thoſe fixed bodies are placed will permit; for at that height of the charge, the exploſion will always be made between them. See Mr. Lane's more particular deſcription of this uſeful inſtrument *.

To the account of theſe articles of an electrical apparatus, which muſt be uſed within doors, it will not be wholly inſignificant to add, that a ſtrong firm table is highly requiſite. For if the TABLE on which the apparatus is diſpoſed be apt to ſhake, a great number of experiments cannot be performed to advantage.

IN order to repeat the noble experiment of the ſameneſs of the electric fluid with the matter of lightning, and to make farther obſervations on the electricity of the atmoſphere, the electrician muſt be provided with a MACHINE FOR DRAWING ELECTRICITY FROM THE CLOUDS. For the beſt conſtruction of ſuch a machine, take the following directions. On the top of any building (which will be the more convenient if it ſtand upon an eminence) erect a pole [*a* fig. 2. Pl. I.] as tall as a man can well manage, having on the top of it a ſolid piece of glaſs or baked wood, a foot in length. Let this be covered with a tin or copper veſſel [*b*] in the form of a funnel, to prevent its ever being wetted. Above this let there riſe a long ſlender rod [*c*] terminating in a pointed wire, and having a

* Phil. Tranſ. Vol. lvii. p. 451.

ſmall

ſmall wire twiſted round its whole length, to conduct the electricity the better to the funnel. From the funnel make a wire [*d*] deſcend along the building, about a foot diſtance from it, and be conducted through an open ſaſh, into any room which ſhall be moſt convenient for managing the experiments. In this room let a proper conductor be inſulated, and connected with the wire coming in at the window. This wire and conductor, being completely inſulated, will be electrified whenever there is a conſiderable quantity of electricity in the air; and notice will be given when it is properly charged, either by Mr. Canton's balls, hung to it, or by ſuch a ſet of bells as will be deſcribed hereafter.

To make theſe experiments in perfect ſafety, the electrified wire ſhould be brought within a few inches of a conducting rod, that ſerves to guard the houſe, that the redundant electricity may paſs off that way, without ſtriking any perſon that may happen to ſtand near it. The conductor to guard the houſe ſhould conſiſt of one rod, between one fourth and one half of an inch thick, if it be of iron, but ſmaller if it be braſs or copper, terminating upwards in a ſharp point, about four or five feet above the higheſt part of the building; and below it ſhould, if poſſible, be continued to ſome well or running water. Otherwiſe it ſhould be ſunk ſeveral feet into the ground, at the diſtance of ſome yards from the building. It is of no conſequence whether this conducting rod be faſtened on the inſide, or outſide of the houſe, or how many bendings are made in it.

If the electrician be deſirous of making experiments upon the electricity of the atmoſphere to greater exactneſs, he muſt raiſe a kite, by means of a ſtring in which a ſmall wire is twiſted. The extremity of this line muſt be ſilk, and the wire muſt terminate in ſome metallic conductor, of ſuch a form as ſhall be thought moſt convenient. Mr. Romas's experiment will perhaps convince my reader, that it may be dangerous to raiſe this kite at the approach of a thunder ſtorm; and upon this occaſion the common apparatus above deſcribed for drawing electricity from the clouds will, probably, anſwer this purpoſe well enough.

But, with the following apparatus, I ſhould apprehend no great danger in any thunder ſtorm. Let the ſtring of the kite [*a* fig. 3. Pl. I.] be wound upon a reel [*b*] going through a ſlit in a flat board, faſtened at the top of it; by which more or leſs of the ſtring may be let out at pleaſure. Let the reel be fixed to the top of a tin or copper funnel [*c*] ſuch as was deſcribed above; and from the funnel let a metallic rod [*d*] with a large knob be projected, to ſerve for a conductor. This funnel and reel muſt be ſupported by a ſtaff [*e*] the upper end of which, at leaſt, muſt be well baked; and the lower end may be made ſharp, to thruſt into the ground, when the kite is well raiſed.

THE

The ſafety of this apparatus depends upon the chain [*f*] faſtened to the ſtaff, by a hook a little below the funnel, and dragging on the ground: for the redundant lightning will ſtrike from the funnel to the chain, and ſo be conducted as far as any one chuſes, without touching the perſon who holds the ſtaff.

Sparks may be taken from the conductor belonging to this apparatus with all ſafety, by means of a ſmall rod of baked wood [*a* fig. 4.] furniſhed with a ſmall funnel [*b*] and a braſs rod [*c*] and a chain connected with: for the lightning which ſtrikes the rod, will paſs by the funnel and the chain, without touching the perſon who holds the rod.

Mr. Hartman, in the conſtruction of his apparatus for obſerving the electricity of the atmoſphere with ſafety, makes uſe of long ſilken ſtrings to ſupport his metallic rod. Theſe, therefore, require a large ſhed, faſtened to the rod above them, to keep them dry. And, leſt the rain that falls upon this ſhed ſhould carry off all the electricity, he makes a channel all round, which receives the rain; and thence he conveys it, under the ſhed, into an inſulated receptacle*. But I cannot help thinking this complex apparatus unneceſſary, eſpecially if a ſolid ſtick of glaſs and a ſmall cover be uſed, inſtead of the ſilken ſtrings and large ſhed.

* Anmerkungen, &c. p. 38.

SECTION

SECTION II.

A DESCRIPTION OF SOME PARTICULAR ELECTRICAL MACHINES, WITH OBSERVATIONS ON THEIR PRINCIPAL ADVANTAGES AND DEFECTS.

AFTER this general account of the conſtruction of electrical machines, and the principal parts of an electrical apparatus, my reader may perhaps expect a more particular account of ſome of the principal varieties with which they are uſually made. And though it may be preſumed, from what has been advanced upon that head, that any perſon might judge for himſelf, I ſhall endeavour to gratify thoſe who are willing to provide themſelves with an electrical machine, by giving drawings and deſcriptions of ſome of the beſt conſtructions that have fallen under my notice, obſerving what I apprehend to be their ſeveral advantages and defects.

I SHALL begin with Mr. Haukſbee's machine [Plate IV. fig. 1.] which is an excellent conſtruction conſidering the ſtate of electricity in his time. The drawings annexed will render a very particular deſcription of this, or the other machine, unneceſſary. This has no rubber, no prime conductor, or field for making experiments; for no ſuch things were wanted in his time: but it may be eaſily accommodated

modated with them all. A conductor may hang from the cieling, a rubber may be supported by a ſpring fixed under the globe, and a table placed near the machine, may receive the apparatus neceſſary for making experiments. The inconveniencies of this conſtruction are, that the operator cannot well turn the wheel himſelf. A ſervant is therefore neceſſary, who muſt ſit to his work. The machine only admits of one globe, or cylinder, which muſt have two necks; though it admits of a conſiderable variety of ſuch, and it is by no means portable.

The Abbé Nollet's machine [Plate IV. fig. 2.] reſembles the greateſt number of the electrical machines that were uſed about the time that the Leyden phial was diſcovered. Theſe were the machines, heavy and unweildy as they ſeem, that were generally carried from place to place, when electrical experiments made a gainful buſineſs, and would bear the expence of the conveyance.

In thoſe early times, electricians had no idea, that it was poſſible to make the globe revolve too ſwiftly. They, therefore, made their wheels exceedingly large, and the frame of the machine proportionably ſtrong. The globe was generally rubbed by the hand, the conductor was a bar of iron, or generally a gun barrel, ſuſpended in ſilken lines from the top of the room, and the apparatus was diſpoſed on an adjoining table.

These machines are now univerſally laid aſide, being more fit for a large laboratory than a private ſtudy. Beſides, they neceſſarily require an aſſiſtant, and do not admit of half the variety in the diſpoſition of the principal parts of the conſtruction, which the variety of experiments now demands.

About the time that Mr. Boze's beatification was talked of, electricians were very deſirous of exciting a very great power of electricity; though, having no method of accumulating, or preſerving it, it was diſperſed as ſoon as raiſed. The machine repreſented in Plate V. fig. 1. was a contrivance of Dr. Watſon's, to whirl four large globes at a time, and unite the power of them all.

I cannot help regretting that no ſuch machines as theſe are conſtructed at this day, when, by means of electrical batteries, ſo great a power might be preſerved, and employed to the greateſt purpoſes. I wiſh the Doctor would refit the machine here deſcribed, if it be yet in being, and conſtruct a battery proportioned to it. But I ſhould rejoice more to ſee a machine moving by wind or water, turning twenty or thirty globes, and charging electrical batteries adequate to them. I make no doubt but that a full charge of two or three thouſand ſquare feet of coated glaſs would give a ſhock as great as a ſingle common flaſh of lightning. They are not philoſophers who will ſay, that nothing could be gained, and no new diſcoveries made by ſuch a power.

Plate

PLATE V. fig. 2. exhibits a machine which Mr. Wilſon ccnſtructed, about the time above mentioned. It is much more commodious than any that had been contrived before, as all the parts are brought within a moderate compaſs; ſo that the ſame perſon may turn the wheel, and conduct the experiments.

ITS inconveniences are, that it admits but little variety of globes or cylinders, and both theſe and the rubber are not ſufficiently diſtant from other bodies. The rubber is not inſulated, and the conductor is unſteady. This machine has a frame ſtanding upon the ground, but the general conſtruction may be preſerved, and the machine be made to ſcrew to a table. Some I have ſeen which, by this means, were made very portable; and a box was contrived in the inſide, to contain the apparatus.

OF the more modern conſtructions (of which there is an endleſs variety) the more elegant are thoſe in which the globe is turned by tooth and pinion. This reduces the wheel work, contained in the box [*a* Pl. VI. fig. 1.] to an exceedingly ſmall compaſs, and gives the workmen an opportunity of making the machine all in braſs, very elegant, and portable. But I object to them, as liable to accidents, which electricians in general cannot eaſily repair; and I would wiſh philoſophers to be as independent as poſſible of all workmen. The conductor belonging to machines of this conſtruction is generally hung in ſilk, ſupported either by wooden pillars in a frame, as in the figure annexed, or by two braſs arms extending from the machine.

These machines are certainly very commodious for ſcrewing to a table. They require no aſſiſtant, and they admit of the experiments being made in a ſitting poſture; which is a great recommendation of a machine, to thoſe perſons who chuſe to do things with little trouble, and who are fond of a ſtudious ſedentary life. This conſtruction admits of very little variety in the ſize or number of globes, and hardly of a glaſs veſſel of any other form. But the greateſt inconvenience attending it, is the upright poſition of the globe and rubber, whereby every thing put upon it is apt to ſlip down; and the rubber is not inſulated.

In the machine repreſented in Plate VI. fig. 2. and which was invented by Mr. Read, mathematical inſtrument maker at the quadrant in Knightſbridge, Hyde-park, a cylinder ſtands perpendicular to the horizon, ſupported by a braſs bow, which receives the upper end of the axis; and motion is given to it by means of a pulley at the lower end of the axis, and a wheel which lies parallel to the table. The conductor [*a*] is furniſhed with points to collect the fire, and it is ſcrewed to the wire of a coated jar [*b*], ſtanding in a ſocket, between the cylinder and the wheel. One machine of this kind I have ſeen, in which the cylinder and the wheel were not ſeparated by the conductor.

This

This construction is peculiarly useful to physicians and apothecaries; and, with Mr. Lane's electrometer [*c*] annexed to it (the figure of which he has given me leave to insert in the drawing annexed, taken from his own machine) as many shocks as are requisite may be given, of precisely the same, and any degree of force, without any change of posture, either in the patient or the operator, who has nothing to do but turn the wheel, without so much as touching any other part of the apparatus.

When this machine is used for simple electrification, and other purposes where the shock is not required; the coated jar must be taken away, and another jar, without any coating, put in its place. By this means the conductor is fixed, which is a very great advantage, and which few machines are possessed of. But these machines, besides that they admit no variety of globes or cylinders, and no insulation of the rubber, require a motion of the arm, which I should think not quite easy.

The ingenious Dr. Ingenhousz of Vienna, and also Mr. Ramsden, mathematical instrument maker in the Haymarket, each independent of the other, constructed a machine in which friction is not given to any kind of hollow glass vessel whatever; but to a *circular plate of glass*, generally about nine inches in diameter. This plate turns vertically, and rubs against four cushions, each an inch and a half long, placed at the opposite ends of the vertical diameter. The conductor is a brafs tube, has two horizontal branches coming from it, reaching within about half an inch of the extremity of the glafs, fo that each branch takes off the electricity excited by two of the cufhions.

THIS conftruction is original and ingenious, but the cufhions cannot eafily be infulated, and a plate of glafs is much more liable to injuries than a globe, or even a cylinder.

MR. WESLEY'S people, I believe, generally ufe a machine in which two cylinders are turned by the fame wheel: but one that I faw, in the poffeffion of a very intelligent perfon of that perfuafion, had the cylinders and rubbers fo confined in a cheft, that, though it might do very well for medical ufes, it was very ill adapted to the purpofes of philofophy.

BUT the machine which I would advife a philofopher to conftruct for his own ufe, is that of which a drawing is given, Plate VII. This conftruction is the refult of my beft attention to this fubject. I have ufed it above fix months (how much I leave the reader to imagine) without feeing the leaft reafon to make any alteration of confequence in it; and believe it to have almoft all the advantages, which an electrical machine defigned for the clofet can have. The reader will, therefore, allow me to be a little longer in the defcription of it than I have been of the reft.

THE

The frame consists of two strong boards of mahogany [*a a*] of the same length, parallel to one another, about four inches asunder; and the lower an inch on each side broader than the upper. In the upper board is a *groove*, reaching almost its whole length. One of the *pillars* [*b*], which are of baked wood, is immoveable, being let through the upper board, and firmly fixed in the lower, while the other pillar slides in the groove above mentioned, in order to receive globes or cylinders of different sizes; but it is only wanted when an axis is used. Both the pillars are perforated with *holes* at equal distances, from the top to the bottom; by means of which globes may be mounted higher or lower according to their size; and they are tall, to admit the use of two or more globes at a time, one above the other. Four of a moderate size may be used, if two be fixed on one axis: and the wheel has several grooves for that purpose.

If a globe with only one neck be used, as in the plate, a brass arm with an open socket [*c*] is necessary to support the axis beyond the pulley; and this part is also contrived to be put higher or lower, together with the brass socket in which the axis turns. The axis [*d*] is made to come quite through the pillar, that it may be turned by another handle, without the wheel, if the operator chuses. The frame, being screwed to the table, may be placed nearer to, or farther from the wheel, as the length of the ſtring requires, in different ſtates of the weather. The WHEEL is fixed in a frame by itſelf, [*e*] by which it may have any ſituation with reſpect to the pulley, and be turned to one ſide, ſo as to prevent to the ſtring from cutting itſelf. The hinder part of this frame is ſupported by a foot of its own.

THE RUBBER [*f*] conſiſts of a hollow piece of copper, filled with horſe hair, and covered with a baſil ſkin. It is ſupported by a ſocket, which receives the cylindrical axis of *a round plate of glaſs* [*g*], the oppoſite part of which is inſerted into the ſocket of a bent ſteel ſpring [*h*]. Theſe parts are eaſily ſeparated, ſo that the rubber, or the plate of glaſs that ſerves to inſulate it, may be changed at pleaſure. The SPRING admits of a twofold alteration of poſition. It may be either ſlipped along the groove, or moved in the contrary direction, ſo as to give it every deſirable poſition with reſpect to the globe or cylinder; and it is, beſides, furniſhed with a *ſcrew* [*i*], which makes it preſs harder or lighter, as the operator chuſes.

THE PRIME CONDUCTOR [*k*] is a hollow veſſel of poliſhed copper, in the form of a pear, ſupported by a pillar, and a firm baſis of baked wood, and it receives its fire by means of a *long arched wire, or rod of very ſoft braſs* [*l*], eaſily bent into any ſhape, and raiſed higher or lower, as the globe requires; and it is terminated by an *open ring*, in

which

which are hung *ſome ſharp pointed wires* [*m*] playing lightly on the globe when it is in motion. The body of the conductor is furniſhed with *holes*, for the inſertion of metallic rods, to convey the fire wherever it is wanted, and for many other purpoſes convenient in a courſe of electrical experiments. The conductor is, by this means, ſteady, and yet may be eaſily put into any ſituation. It collects the fire perfectly well, and (what is of the greateſt conſequence, though but little attended to) retains it equally every where.

When poſitive electricity is wanted, a wire, or chain, as is repreſented in the plate [*n*] connects the rubber with the table or the floor. When negative electricity is wanted, that wire is connected with another conductor ſuch as that repreſented [*t*, Pl. II.] while the conductor in Plate VII. is connected by another wire or chain, with the table. If the rubber be made tolerably free from points, the negative power will be as ſtrong as the poſitive. In this machine I do not know which is the ſtronger of the two.

In ſhort, the capital advantages of this machine are, that glaſs veſſels, or any other electric body, of any ſize or form, may be uſed, with one neck, or two necks at pleaſure; and even ſeveral of them at the ſame time, if required. All the eſſential parts of the machine, the *globe*, the *frame*, the *wheel*, the *rubber*, and *conductor*, are quite ſeparate; and the poſition of them to one another may be varied in every manner poſſible. The rubber has a complete inſulation, by which means the operator may command either the negative or the poſitive power, and may change them in an inſtant. The conductor is ſteady, and eaſily enlarged, by rods inſerted into the holes, with which it is furniſhed, or by the conjunction of other conductors, in order to give larger ſparks, &c. The wheel may be uſed or not at pleaſure; ſo that the operator may either ſit, or ſtand to his work, as he pleaſes; and he may, with the utmoſt eaſe, both manage the wheel and his apparatus.

The machine repreſented in [Pl. VIII.] is conſtructed on the ſame general principles with the laſt. It is inferior to it in one reſpect, that it admits only of globes or cylinders with one neck, but theſe are far preferable to any other; and it is much more commodious for uſe, as it doth not require any ſtrong table like the other. It conſiſts of a pillar of mahogany [*a*] ſtanding upright on three feet. This pillar divides in two places, to receive a wheel [*b*] in the lower part of it, and in the upper part a pulley [*c*] which is turned by a leathern ſtrap [*d*] tightened by means of a ſmall buckle. In the center of the pulley is a ſtrong iron ſpindle, turning in two firm braſs ſockets, faſtened to each ſide of the pillar. In one of theſe ſockets the extremity of the ſpindle turns upon a center, by means of a piece of iron [*e*] ſcrewed into it, while the other is held tight by a braſs claſp, which may

may be made to hold it clofer, or more loofely, at pleafure, by means of a fcrew [*f*]. The iron fpindle terminates in a male fcrew, anfwering to a female fcrew in the brafs cap of the globe [*g*]; and by this means any globe may be taken out, and another put into the machine with very little trouble, if thefe parts be always made to the fame pattern.

The rubber [*h*] is feparated from the fpring [*i*] by a plate of glafs [*j*], which effectually infulates it; but the chain [*k*] connects them together when pofitive electricity is wanted, as in the ufual method of electrifying. The fpring may be made to prefs more or lefs, by means of a fcrew [*l*]; and it may be raifed higher or lower, to fuit globes of different fizes, by means of a contrivance which is not reprefented in the plate.

The prime conductor *m*, *n*, *o*, is the fame as in Pl. VII. From the fame board which fupports it, arifes another pillar, at the top of which is Mr. Lane's electrometer; the knob of which [*p*] may be placed as near to the knob oppofite to it on the prime conductor [*q*] as is defired, by means of the graduated part [*r*]. But the whole of this may be taken away when it is not wanted.

When negative electricity is defired, the chain [*k*] muft be removed from the rubber, and hung upon the prime conductor, fo as to connect it with the table; and a fhort brafs, rod, with a knob at the end of it, muft be fcrewed into a fmall focket, which will be

 found

found in the rubber above the plate of glaſs. This braſs rod will then ſerve for a negative prime conductor; for, in this ſituation, when the wheel of the machine is turned, this rod, being inſulated (together with the rubber, through which all the electric fire paſſes to the globe) will receive ſparks from whatever is preſented to it, and therefore electrify negatively.

As it requires ſome dexterity and experience to turn the machine, ſtanding on three feet only, without ſhaking it; ſmall plates of braſs, upon which the edges of heavy weights, made of lead or iron, may be placed, are faſtened to two of them; but a large board may be firmly ſcrewed under all the feet, or various other methods may be uſed, whereby the pillar, which ſupports the machine, may ſtand as firm as a perſon chuſes.

PART VI.

PRACTICAL MAXIMS FOR THE USE OF YOUNG ELECTRICIANS.

AS the chapter I am now entering upon is profeſſedly deſigned for the uſe of young electricians, it is hoped that the proficient will excuſe my inſerting a few plain and trite maxims; which, though they be ſuperfluous with reſpect to him, may not be ſo to all my readers. The greateſt electricians (who are generally thoſe who have had the feweſt inſtructions) may remember the time when the knowledge of a rule or maxim, which they would perhaps ſmile to ſee in a book, would have ſaved them a great deal of trouble and expence; and it is hoped they will not envy others acquiring wiſdom cheaper than they did. In a general treatiſe, every man has an equal right to expect to find what he wants; and it is for the intereſt of the ſcience in general, that every thing be made as eaſy and inviting as poſſible to beginners It is this circumſtance only that can increaſe the number of electricians, and it is from the increaſe of this number that we may moſt reaſonably expect improvements in the ſcience.

When the air is dry, particularly when the weather is frosty, and when the wind is North, or East, there is hardly any electrical machine but will work very well. If the air be damp, let the room in which the machine is used be well aired with a fire, and let the globe and every thing about it be made very dry, and it may be made to work almost as well as in the best state of the air.

When a tube is used, the hand should be kept two or three inches below the upper part of the rubber; otherwise the electricity will discharge itself upon the hand, and nothing will remain upon the tube for electrical purposes.

A little bees wax drawn over the surface of a tube will greatly increase its power. When the tube is in very good order, and highly excited, it will, at every stroke, throw off many pencils of rays from its surface, without the approach of any conductor, except what may float in the common atmosphere.

It has been the custom of many electricians to line their globes with sealing wax, or some other electric substance, in order to make them act with more ease and vigour. Mr. C. L. Epinasse gives the following receipt for an electric composition for this purpose. Take four pounds of Venice turpentine, one pound of rosin, and one pound of bees wax. Boil these over a gentle fire, stirring them now and then, for four hours; at the end of which stir in one quarter of a pound of vermilion. Then

a little

a little of the mixture being taken out and left to cool, will be hard and brittle, a token that it is fit for uſe. Having well heated your globe or cylinder, pour the melted mixture into it, and turn it about, ſo as to ſpread it evenly over the inſide ſurface, to the thickneſs of a ſixpence, and let it cool very gradually *.

I MAKE no doubt but that this electric lining is uſeful in ſome caſes, eſpecially in keeping the inſide of a globe free from moiſture, which is more apt to adhere to glaſs than other electric ſubſtances. It will be ſeen that a lining of ſulphur was remarkably uſeful in the caſe of two large globes of my own, the hiſtory of which will be given in the laſt part of this work.

To increaſe the quantity of electric fire from a globe, moiſten the rubber a little from time to time; or rather moiſten the under ſide of a looſe piece of leather, which may occaſionally be put upon the rubber. But the moſt powerful exciter of electricity is a little amalgam, which may be made by rubbing together mercury and thin pieces of lead or tinfoil in the palm of the hand. If the rubber ſhould be placed perpendicular to the horizon, it will be neceſſary to uſe a little tallow to make it ſtick. With this excellent reſource, almoſt all ſtates of the weather are equal to an electrician.

* Phil. Tranſ. Vol. lvii. p. 186.

A LITTLE

A LITTE time after freſh amalgam has been put upon the globe, and often at other times, if there be any foulneſs upon the cuſhion; and ſometimes when there is none, there will be formed upon the globe ſmall black ſpots, of a hard rough ſubſtance, which grow continually larger, till a conſiderable quantity of that matter be accumulated upon the ſurface. This muſt be carefully picked off, or it will obſtruct the excitation, and in a great meaſure defeat the electrical operations.

WHEN the amalgam has been uſed for ſome time, there will be formed upon the rubber a thick incruſtation of the ſame kind of black ſubſtance which is apt to adhere to the globe. This incruſtation is a very great improvement of the rubber. For when once a conſiderable body of this matter is formed, and it is a little moiſtened, or ſcraped, as much fire will be produced, as if freſh amalgam were uſed: ſo that it ſeems almoſt to ſuperſede the farther uſe of the amalgam.

As the lectric matter is only collected at the rubber, it is neceſſary that it have a communication with the common maſs of the earth, by means of good conductors. If, therefore, the table on which the machine ſtands, or the floor of the room in which it is uſed, be very dry, little or no fire will be got, be the machine ever ſo good. In this caſe it will be neceſſary to connect the rubber, by means of chains or wires, with the floor, or even the next water, if the neighbouring ground

be

be dry. This Dr. Franklin informs me he was frequently obliged to do in Philadelphia.

When the electricity of a globe is very vigorous, the electric fire will feem to dart from the cufhion towards the wire of the conductor. I have feen thofe lucid rays (which are vifible in day-light) make the circuit of half the globe, and reach the wires: and they will frequently come in a confiderable number, at the fame time, from different parts of the cufhion. The noife attending this beautiful phenomenon exactly refembles the crackling of bay leaves in the fire. Frequently thefe lucid arches have *radiant points*, often four or five in different parts of the fame arch. Thefe radiant points are intenfely bright, and appear very beautiful. It is peculiarly pleafing to obferve thefe circles of fire rife from thofe parts of the cufhion where the amalgam or moifture has been put, or which have been lately fcraped. Single points on the rubber will then feem intenfely bright, and for a long time together will feem to pour out continual torrents of flame. If one part of the rubber be preffed clofer than another, the circles will iffue in that place more frequently than in any other.

When the conductor is taken quite away, circles of fire will appear on both fides the rubber, which will fometimes meet, and completely encircle the globe. If a finger be brought within half an inch of the globe, in that ftate, it is fure to be ftruck very fmartly, and

and there will often be a complete arch of fire from it to the rubber, though it be almost quite round the globe.

The ſmaller the conductor is made, the more fire may be collected from it: for there is leſs ſurface from which the fire may eſcape. But in charging a phial, if the wire be placed cloſe to the conductor, the difference will be inconſiderable, whether a ſmaller or larger conductor be uſed, till it begin to be charged pretty high; for, till that time, the conductor will not have acquired any conſiderable atmoſphere.

If the conductor be made perfectly well, and the air be dry, there will never be any loſs of fire from any part of it. For when the whole ſurface has received as high a charge as the machine can give to it, it will, in all places alike, perfectly reſiſt all farther efforts to throw more upon it, and the circulation of the fluid by the rubber will be ſtopped, being balanced, as it were, by equal forces. Or if it loſe in all places alike, the diſſipation muſt be inviſible. This maxim almoſt admits of ocular demonſtration. For when the rubber is perfectly inſulated, and the conductor has an opportunity of diſcharging itſelf, the rubber will take ſparks from a wire placed near it very faſt; but when the conductor has little opportunity of emptying itſelf, it will take fewer of thoſe ſparks.

To form a juſt eſtimate of the electrical power of any machine, and to compare different machines in this reſpect, take two wires with

with knobs of any ſize, and fix one of them at the conductor, and the other at ſome certain diſtance from it, about an inch, or an inch and a half; and when the wheel is turned, count the number of ſparks that paſs between them during any given time. Fix the ſame wires to any other conductor, belonging to any other machine (but the ſame conductor would be more exact) and the difference between the number of ſtrokes in any given time will aſcertain the difference between the ſtrength of the two machines.

THE larger the conductor is made, the ſtronger ſpark it will give: for the more extended the electrified ſurface is, the greater quantity of the electric atmoſphere it contains, and the more ſenſible will be its effects when it is all diſcharged at once. The conductor, however, may be made ſo large, that the neceſſary diſſipation of the electric matter from its ſurface into the air will be equal to the ſupply from the machine, which will conſtitute the MAXIMUM of the power of that machine, and will be different in different ſtates of the air.

A CERTAIN degree of friction is neceſſary to give a globe its greateſt power. A number of globes increaſes the power, but the increaſe of friction will make it more difficult for a man to excite their power. A few trials with a number of globes would enable any man to judge of the *maximum* of his ſtrength in exciting electricity. I ſhould imagine, from my own experience, that no perſon could excite much

much more electricity from any number of globes, than he could from one; supposing him to continue the operation an hour, or even only half an hour together.

When a long conductor is used, the longest and the strongest spark may be drawn from the extremity of it, or from that part which is the most remote from the globe.

Very large and pungent sparks are often drawn from any conductor along an electric substance. Thus if the conductor be supported by pillars of glass or of baked wood, these sparks will be taken close to the pillar.

If the conductor bend inwards in any place, so as to make the surface concave, a peculiarly large, strong, and undivided spark may be drawn from that place. Where the surface is convex, the spark is more apt to be divided and weakened.

If a smooth cork ball be hung in a long silken string, and electrified positively, it will always be repelled by positive, and attracted by negative electricity. But the strongest repulsion will be changed into attraction at a certain distance.

If two pith balls hung by linen threads, and diverging with positive electricity, be insulated, though in connection with conductors of considerable length, the approach of a body electrified positively will first make them separate, and then (if the electricity of the balls be small, and that of the approaching body great) it will, at a certain distance, make them approach, and at length come into

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to contact with it. Sometimes the divergence previous to the convergence is very ſlight, and, without great attention, is apt to be overlooked.

If the balls have a free communication with the earth, for inſtance, if they be held in the hand of a perſon ſtanding on the ground, and (as in the former caſe) they be made to diverge with poſitive electricity, in conſequence of being held within the influence of a body electrified negatively, the approach of poſitive electricity will make them converge, and negative electricity will make them diverge: the electric matter of the approaching body, in the former caſe, repelling that of the balls, and thereby, as it were, unelectrifying them; whereas, in the latter caſe, the negative electricity of an approaching body draws it more powerfully into the threads, and makes them diverge more. This method of judging is, therefore, excellently adapted to aſcertain the kind of electricity in the atmoſphere, or of a charged jar or battery, the balls being held in the hand of a perſon ſtanding on the earth or the floor.

To diſcover ſmaller degrees of electricity than the balls can ſhow to advantage, uſe a very fine thread, or two of them. If inſulation be neceſſary, faſten it to a ſtick of baked wood. But the moſt accurate meaſure of electricity I have yet hit upon, is a ſingle thread of ſilk as it comes from the worm. When the end of this has received a ſmall degree of electricity, it will retain it a conſiderable time, and the ſlighteſt electric force will give it motion Before any experiments be made, let it be carefully obſerved how long, in any particular ſituation, it will retain the degree of electricity that is intended to be given to it; and let allowance be made for that in the courſe of the experiments. It will retain electricity much longer, if a ſmall piece of down from a feather be faſtened to it, but it will not acquire the virtue ſo ſoon. And it will be moſt eaſy to manage, if two or three threads of ſilk be uſed, and the piece of down be ſo adjuſted to them, that it ſhall but juſt prefer a perpendicular ſituation, and not abſolutely float in the air at random. This electrometer is not liable to the inaccuracies of thoſe that have a ſenſible weight: for as there is always a ſphere of attraction within a ſphere of repulſion, the weight of the electrified body will allow another to paſs the boundary of thoſe two ſpheres, without a ſenſible obſtruction; but the body I am deſcribing immediately retires, with all its ſpheres of attraction and repulſion about it.

THE force of the ELECTRIC SHOCK is in proportion to the quantity of ſurface coated, the thinneſs of the glaſs, and the power of the machine. That this laſt circumſtance ought to be taken into conſideration is evident: for different machines will charge the ſame jar very differently. With one machine, for inſtance, it may be made to diſcharge itſelf, when it cannot with another.

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The moſt effectual method of charging a jar is to connect the outſide, by means of wires, with the rubber, while the wire proceeding from the inſide is in contact with the conductor. In this manner the inſide of the jar will be ſupplied with the very ſame fire that left the outſide. In this caſe alſo the jar will receive as high a charge as it is capable of receiving, though the rubber be inſulated, and have no communication but with the outſide coating; ſo that, in the caſe of charging, there can be no occaſion for the directions given above, when the table, the floor of the room, or the ground are very dry.

The greateſt quantity of fire that a jar will hold is not always the quantity it will contain when it is coated juſt ſo low as not to diſcharge itſelf. In this caſe, indeed, the part that is coated is charged as high as it can be, but then a conſiderable part of the ſurface is not charged at all, or very imperfectly. On the other hand, if the jar be coated very high, it may be made to diſcharge itſelf with as ſmall an exploſion as one chuſes. The exact *maximum* of the charge of any jar is not eaſy to aſcertain.

The greateſt effort in a jar to make a diſcharge ſeems to be about half a minute, or a minute, after it is removed from the conductor, owing, perhaps, to non-electric duſt or moiſture attracted by and adhering to the glaſs, between the outſide and inſide coating; ſo that if there be any apprehenſion of its diſcharging itſelf, it is adviſeable to diſcharge it before it has ſtood charged at all.

When a thin jar is diſcharged, it is adviſable not to do it by placing the diſcharging rod oppoſite to the thinneſt part. It will endanger the burſting of the jar in that place.

The more perſons join hands to take a ſhock the weaker it is.

If two jars, of the ſame thickneſs, be uſed together, the ſtronger of them will receive no higher a charge than the weaker. If one of them, for inſtance, be coated ſo high as that it will diſcharge itſelf, either with or without burſting, after a few turns of the wheel; the other will always be diſcharged along with it, though it was capable of being charged ever ſo high by itſelf. The method, therefore, of eſtimating the force of a number of jars, is to conſider each of them as capable of containing no more fire than the weakeſt in the company. It follows from hence, that if a ſingle jar in a large battery have the ſmalleſt crack in the coated part of it, not one of them is capable of being charged in conjunction with it.

In large batteries, it is adviſable to coat the jars pretty high, the diſſipation of the electric matter from ſo great a ſurface when the charge is high being very conſiderable. The battery might be made ſo large, as that after a very moderate charge, the machine would be able to throw no more fire in than was

was exhaled, as we may ſay, from the ſurface. This would be the MAXIMUM OF THE POWER OF THAT MACHINE IN CHARGING.

IN order to judge of the ſtrength of a charge (which, in large batteries, is a thing of conſiderable conſequence) preſent Mr. Canton's balls to the wires, from time to time. A compariſon of the degree of their divergence, compared with the actual exploſion, will ſoon enable the operator to tell how high his battery is charged, and what will be the force of the exploſion.

IN comparing different exploſions by their power to melt wires, let it be obſerved, that, in wires of the ſame thickneſs, the forces that melt them will be in the proportion of the lengths; and in wires of the ſame length, in the proportion of the ſquares of their diameters.

Do not expect that the exploſion of a battery will pierce a number of leaves of paper in proportion to its force in other reſpects. That depends upon the height of the charge much more than the quantity of coated ſurface. I have known an exploſion which would have melted a pretty thick wire not able to pierce the cover of a book, which a ſmall common jar would have done with eaſe. If it had been pierced with the exploſion of the battery, the hole would have been larger in proportion.

Let no perſon imagine that, becauſe he can handle the wires of a large battery without feeling any thing, that therefore he may ſafely touch the outſide coating with one hand, while the other is upon them. I have more than once received ſhocks that I ſhould not like to receive again, when the wires ſhowed no ſign of a charge; even two days after the diſcharge, and when papers, books, my hat, and many other things had lain upon them the greateſt part of the time. If the box be tolerably dry, the reſiduum of the charge will not diſperſe very ſoon. I have known even the *reſiduum of a reſiduum* in my batteries to remain in them ſeveral days. For preſently after an exploſion, I ſeldom fail to diſcharge the reſiduum, which, in ſome caſes, is very conſiderable, for fear of a diſagreeable accident.

A small ſhock paſſing through the body gives a ſenſation much more acute and pungent than a large one. I cannot boaſt, like Dr. Franklin, of being twice ſtruck ſenſeleſs by the electric ſhock; but I once, inadvertently, received the full charge of two jars, each containing three ſquare feet of coated glaſs. The ſtroke could not be called painful, but, though it paſſed through my arms and breaſt only, it ſeemed to affect every part of my body alike. The only inconvenience I felt from it was a laſſitude, which went off in about two hours.

Mr.

Mr. Wilcke was ſtruck down ſenſeleſs, by accidentally receiving, from his head to his feet, the charge of a large chymical receiver. He thought, that if he had received a ſhock five times as large, he ſhould never have written the account of it *.

* Remarks on Franklin's Letters, p. 362.

PART VII.

A DESCRIPTION OF THE MOST ENTERTAINING EXPERIMENTS PERFORMED BY ELECTRICITY.

ELECTRICITY has one conſiderable advantage over moſt other branches of ſcience, as it both furniſhes matter of ſpeculation for philoſophers, and cf entertainment for all perſons promiſcuouſly. Neither the air pump, nor the orrery; neither experiments in hydroſtatics, optics, or magnetiſm; nor thoſe in all other branches of Natural Philoſophy ever brought together ſo many, or ſo great concourſes of people, as thoſe of electricity have done ſingly. Electrical experiments have, in almoſt every country in Europe, occaſionally furniſhed the means of ſubſiſtence to numbers of ingenious and induſtrious perſons, whoſe circumſtances have not been affluent, and who have had the addreſs to turn to their own advantage that paſſion for the marvellous, which they ſaw to be ſo ſtrong in all their fellow-creatures. A man need not deſire a greater income than the ſums which have been received in ſhillings, ſix-pences, three-pences, and two-pences, for exhibiting the Leyden experiment.

If we only conſider what it is in objects that makes them capable of exciting that pleaſ-
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ing aſtoniſhment, which has ſuch charms for all mankind, we ſhall not wonder at the eagerneſs with which perſons of both ſexes, and of every age and condition, run to ſee electrical experiments. Here we ſee the courſe of nature, to all appearance, intirely reverſed, in its moſt fundamental laws, and by cauſes ſeemingly the ſlighteſt imaginable. And not only are the greateſt effects produced by cauſes which ſeem to be inconſiderable, but by thoſe with which they ſeem to have no connection. Here, contrary to the principles of gravitation, we ſee bodies attracted, repelled, and held ſuſpended by others, which are ſeen to have acquired that power by nothing but a very ſlight friction; while another body, with the very ſame friction, reverſes all its effects. Here we ſee a piece of cold metal, or even water, or ice, emitting ſtrong ſparks of fire, ſo as to kindle many inflammable ſubſtances; and *in vacuo* its light is prodigiouſly diffuſed and copious, ſo as exactly to reſemble, what it really is, the lightning of heaven. Again, what can ſeem more miraculous than to find, that a common glaſs phial or jar, ſhould, after a little preparation (which, however, leaves no viſible effect, whereby it could be diſtinguiſhed from other phials or jars) be capable of giving a perſon ſuch a violent ſenſation, as nothing elſe in nature can give, and even of deſtroying animal life; and this ſhock attended with an exploſion like thunder, and a flaſh like that of lightning? Laſtly, what would the ancient philo-

philoſophers, what would Newton himſelf have ſaid, to ſee the preſent race of electricians imitating in miniature all the known effects of that tremendous power, nay diſarming the thunder of its power of doing miſchief, and, without any apprehenſion of danger to themſelves, drawing lightning from the clouds into a private room, and amuſing themſelves at their leiſure, by performing with it all the experiments that are exhibited by electrical machines.

So far are philoſophers from laughing to ſee the aſtoniſhment of the vulgar at theſe experiments, that they cannot help viewing them with equal, if not greater aſtoniſhment themſelves. Indeed, all the electricians of the preſent age can well remember the time, when, with reſpect to theſe things, they themſelves would have ranked among the ſame ignorant ſtaring vulgar.

Besides, ſo imperfectly are theſe ſtrange appearances underſtood, that philoſophers themſelves cannot be too well acquainted with them; and therefore ſhould not avoid frequent opportunities of ſeeing the ſame things, and viewing them in every light. It is poſſible that, in the moſt common appearances, ſome circumſtance or other, which had not been attended to, may ſtrike them; and that from thence light may be reflected upon many other electrical appearances.

Whether philoſophers may think this conſideration worth attending to or not, I ſhall, for the ſake of thoſe electricians who

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are young enough, and, as it may be thought, childiſh enough, to divert themſelves and their friends with electrical experiments, deſcribe a number of the moſt beautiful and ſurpriſing appearances in electricity; that the young operator may not be at a loſs what to exhibit when a company of gentlemen or ladies wait upon him, and that he may be able to perform the experiments to the moſt advantage, without diſappointing his friends, or fretting himſelf.

To make this buſineſs the eaſier to the young operator, I ſhall conſult his convenience in the order in which I ſhall relate the experiments, beginning with thoſe which only require ſimple electrification, then proceeding to thoſe in which the Leyden experiment is uſed, and concluding with thoſe in which recourſe muſt be had to other philoſophical inſtruments in conjunction with the electrical machine.

SECTION

SECTION I.

Entertaining experiments in which the Leyden phial is not used.

THE phenomena of electrical attraction are shown in as pleasing a manner by the tube, as they can be by any methods that have been found out since the later improvements in electricity. It is really surprising to see a feather, or a piece of leaf gold first attracted by a glass tube excited by a slight friction, then repelled by it, and held suspended in the air, or driven about the room wherever the operator pleases; and the surprise is increased by seeing the feather, which was repelled by the smooth glass tube, attracted by an excited rough tube, or a stick of sealing wax, &c. and jumping from the one to the other, till the electricity of both be discharged. Nor is the observation of Otto Guericke the least pleasing circumstance, viz. that in turning the tube round the feather, the same side of the feather is always presented towards it.

But since electrical substances part with their electricity but slowly, the more rapid alternate attractions and repulsions are shown to the best advantage at the prime conductor. Thus present a number of seeds of any kind, grains

grains of ſand, a quantity of braſs duſt, or other light ſubſtances in a metal diſh (or rather in a glaſs cylindrical veſſel ſtanding on a metal plate) to another plate hanging from the conductor [as at *n* and *o*, Pl. II.] and the light ſubſtances will be attracted and repelled with inconceivable rapidity, ſo as to exhibit a perfect ſhower, which, in the dark will be all luminous.

SUSPEND one plate of metal to the conductor, and place a metal ſtand, of the ſame ſize, at the diſtance of a few inches exactly under it, and upon the ſtand put the figures of men, animals, or whatever elſe ſhall be imagined, cut in paper or leaf gold, and pretty ſharply pointed at both extremities; and then, upon electrifying the upper plate, they will perform a dance, with amazing rapidity of motion, and to the great diverſion of the ſpectators.

IF a downy feather, or a piece of thiſtle down be uſed in this manner, it will be attracted and repelled with ſuch aſtoniſhing celerity, that both its form and motion will diſappear; all that is to be diſcerned being its colour only, which will uniformly fill the whole ſpace in which it vibrates *.

IF a piece of leaf gold be cut with a pretty large angle at one extremity, and a very acute one at the other, it will need no lower plate, but will hang by its larger angle at a ſmall diſtance from the conductor, and by the con-

* Lovet, p. 28.

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tinual waving motion of its lower extremities, will have the appearance of ſomething animated, biting or nibling at the conductor. It is therefore called by Dr. Franklin the *golden fiſh*.

To the dancing figures above mentioned, it is very amuſing to add a ſet of ELECTRICAL BELLS. Theſe conſiſt of three ſmall bells, the two outermoſt of which are ſuſpended from the conductor by chains, and that in the middle by a ſilken ſtring, while a chain connects it with the floor; and two ſmall knobs of braſs, to ſerve inſtead of clappers, hang by ſilken ſtrings, one between each two bells. In conſequence of this diſpoſition, when the two outermoſt bells, communicating with the conductor, are electrified, they will attract the clappers, and be ſtruck by them. The clappers, being thus loaded with electricity, will be repelled, and fly to diſcharge themſelves upon the middle bell. After this, they will be again attracted by the outermoſt bells; and thus by ſtriking the bells alternately, a continual ringing may be kept up as long as the operator pleaſes. In the dark, a continual flaſhing of light will be ſeen between the clappers and the bells; and when the electrification is very ſtrong, theſe flaſhes of light will be ſo large, that they will be tranſmitted by the clapper from one bell to the other, without its ever coming into actual contact with either of them, and the ringing will, conſequently, ceaſe. When theſe two experiments of the bells and the figures are exhibited ed at the ſame time, they have the appearance of men or animals dancing to the muſic of the bells; which, if well conducted, may be very diverting.

If a piece of burnt cork, about the bigneſs of a pea, cut into the form of a ſpider, with legs of linen thread, and a grain or two of lead put in it, to give it more weight, be ſuſpended by a fine ſilken thread, it will, like a clapper between the two bells, jump from an electrified to an unelectrified body and back again, or between two bodies poſſeſſed of different electricities, moving its legs as if it were alive, to the great ſurprize of perſons unacquainted with the conſtruction of it. This is an American invention, and is deſcribed by Dr. Franklin *.

Several very beautiful experiments, which depend on electrical repulſion, may be ſhown to great advantage by bundles of thread, or of hair, ſuſpended from the conductor, or preſented to it. They will ſuddenly ſtart up, and ſeparate upon being electrified, and inſtantly collapſe when the electricity is taken off. If the operator can manage this experiment with any degree of dexterity, the hair will ſeem to the company to riſe and fall at the word of command.

If a large plumy feather be fixed upright on an electrified ſtand, or held in the hand of a perſon electrified, it is very pleaſing to obſerve how it becomes turgid, its fibres extend-

* Letters, p. 17.

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ing themſelves in all directions from the rib; and how it ſhrinks, like the ſenſitive plant, when any unelectrified body touches it, when the point of a pin or needle is preſented to it, or when the prime conductor with which it is connected.

But the effects of electrical repulſion are ſhown in a more ſurpriſing manner by means of water iſſuing out of a capillary tube. If a veſſel of water be ſuſpended from the conductor, and a capillary ſyphon be put into it, the water will iſſue ſlowly, and in the form of large drops from the lower leg of the ſyphon; but, upon electrifying this little apparatus, inſtead of drops, there will be one continued ſtream of water; and if the electrification be ſtrong, a number of ſtreams, in the form of a cone, the apex of which will be at the extremity of the tube; and this beautiful ſhower will be luminous in the dark.

Lastly, Mr. Rackſtrow's experiment (as it is generally called, but which Mr. Henley informs me was really invented by John Serocold, Eſq.) is a ſtriking inſtance of electrical attraction and repulſion, and, at the ſame time, exhibits a very pleaſing ſpectacle. Electrify a hoop of metal, ſuſpended from the prime conductor (or ſupported with ſmall pieces of ſealing wax, &c.) about half an inch above a plate of metal, and parallel to it. Then place a round glaſs bubble, blown very light, upon the plate, near the hoop, and it will be immediately attracted to it. In conſequence of this, the part of the bubble which touched

touched the hoop will acquire ſome electric virtue, and be repelled; and, the electricity not being diffuſed over the whole ſurface of the glaſs, another part of the ſurface will be attracted, while the former goes to diſcharge its electricity upon the place. This will produce a revolution of the bubble quite round the hoop, as long as the electrification is continued; and it will be either way, juſt as it happens to ſet out, or as it is driven by the operator. If the room be darkened the glaſs ball will be beautifully illuminated. Two bubbles may be made to revolve about the ſame hoop, one on the inſide, and the other on the outſide; and either in the ſame, or contrary directions. If more hoops be uſed, a greater number of bubbles may be made to revolve, and thus a kind of *planetarium* or *orrery* might be conſtructed, and a ball hung over the center of all the hoops would ſerve to repreſent the ſun in the center of the ſyſtem. Or the hoops might be made elliptical, and the ſun be placed in one of the *foci*. N. B. A bell or any metallic veſſel inverted would ſerve inſtead of a ſingle hoop.

ALL the motions above mentioned are the immediate effect of electrical attraction and repulſion. The following amuſing experiments are performed by giving motion to bodies through the medium of air, i. e. by firſt putting the air in motion. Let the electrician provide himſelf with a ſet of vanes, made of gilt paper or tinſel, each about two inches in length and one in breadth. Let theſe be

ſtuck

ſtuck in a cork, which may be ſuſpended from a magnet by means of a needle; and then, if they be held at a ſmall diſtance on one ſide of the end of a pointed wire proceeding from the conductor, they will be turned round with great rapidity by the current of the air which flows from the point. If the vanes be removed to the other ſide of the point, the motion will preſently ſtop, and begin again with the ſame rapidity, in a contrary direction; and thus the motion may be changed at pleaſure. This experiment may be diverſified by vanes cut in the form of thoſe of a ſmoke-jack; when, being held over the end of a pointed wire, turned upwards, and electrified, they will be turned round very ſwiftly, by the current of air flowing upwards. If they be held under a point projecting downwards, they will be turned the contrary way.

On the top of a finely pointed wire, riſing perpendicularly from the conductor, let another wire, ſharpened at each end, be made to move freely as on a center. If it be well balanced, and the points be bent horizontally, in oppoſite directions, it will, when electrified, turn very ſwiftly round, by the reaction of the air againſt the current which flows from the points. Theſe points may be nearly concealed, and horſes or other figures placed upon the wires, ſo as to turn round with them, and look as if the one purſued the other. This experiment Mr. Kinnerſley calls the ELECTRICAL HORSE RACE. If the number of wires proceeding from the ſame center be increaſed, creafed, and different figures be put upon them, the race will be more complicated and diverting. If this wire which fupports the figures have another wire finely pointed rifing from its center, another fet of wires, furnifhed with other figures, may be made to revolve above the former, and either in the fame, or in a contrary direction, as the operator pleafes.

If fuch a wire, pointed at each end, and the ends bent in oppofite directions, be furnifhed like a dipping needle with a fmall axis fixed in its middle, at right angles with the bending of the points, and the fame be placed between two infulated wire ftrings, near and parallel to each other, fo that it may turn on its axis freely upon and between them; it will, when electrified, have a progreffive as well as circular motion, from one end of the wires that fupport it to the other, and this even up a confiderable afcent.

A VARIETY of beautiful appearances may be exhibited by means of electrical LIGHT, even in the open air, if the room be dark. Brufhes of light from points electrified pofitively, and not made very fharp, or from the edges of metallic plates, diverge in a very beautiful manner, and may be excited to a great length, by prefenting to them a finger, or the palm of the hand, to which they feel like foft lambent flames, which have not the leaft pungency, nor give a difagreeable fenfation of any kind. It is alfo amufing to obferve the difference there is between brufhes of light from pointed bodies electrified positively or negatively.

In the electrical horſe race above mentioned, a ſmall flame will be ſeen in the dark at every point of the bent wires; ſo that, if the operator can contrive to make the wire terminate in the horſe's tail, it will ſeem to be all on fire. And if a circular plate of metal be cut into the ſhape of a ſtar, ſo that every point may be at the ſame diſtance from the center, and the center be made to turn freely on a point, like the wires in the preceding experiment, a ſmall flame will be ſeen at every point; and if the ſtar be turned round, it will exhibit the appearance of a lucid circle, without any diſcontinuance of the light.

If the electric ſparks be taken from a braſs ball, at the extremity of a long braſs rod, inſerted into the prime conductor, they will often be ſeveral inches long, and iſſue in a great variety of crooked directions, exactly reſembling the courſe of lightning, and exhibiting a very amuſing ſpectacle. A friend of Dr. Franklin's ſuppoſes that the ſpark is thrown out of a ſtrait courſe by the denſity of the air increaſed by the action and reaction of the two fluids, which are repulſive of one another *.

As the motion of the electric matter is, to the ſenſes, inſtantaneous, a variety of beautiful appearances may be exhibited by a num-

* Franklin's Letters, new edition, p. 167.

ber

ber of ſmall electric ſparks, diſpoſed in various forms. This may be done by means of a board and a number of wires, in the following manner. Let two holes be made through the board, about a quarter of an inch on each ſide of the ſpot where a ſpark is deſired. Let the extremities of the wires neatly rounded, come through theſe holes, and be brought near together, exactly over the place; and let the wires on the back ſide of the board be ſo diſpoſed, as that an electric ſpark muſt take them all in the ſame circuit. When they are thus prepared, all the points will appear luminous at once, whenever a ſpark is taken by them at the prime conductor. In this manner may beautiful repreſentations be made of any of the conſtellations, as of the Great Bear, Orion, &c. and in this manner, alſo, may the outlines of any drawing, as of figures in tapeſtry, be exhibited.

The Abbé Nollet has taken a great deal of pains to make the appearance of letters, and other figures, by means of electric ſparks, and as it is impoſſible to make the ſparks follow one another in a complete circle, on the ſame ſide of any flat ſurface, he makes uſe of plates of glaſs, and places one half of the circle, &c. on one ſide of the plate, and the other half on the other ſide, connecting the pieces that are neareſt one another, but on different ſides of the glaſs, by wires brought round it*. The deſcription would be too tedious for this

* Lettres, Vol. iii. p. 281.

place, but the execution will be very eaſy, to any perſon who has but a little knowledge of electricity.

The force of an electric ſpark in ſetting fire to various ſubſtances was one of the firſt experiments that gave an *eclat* to electricity, and it is ſtill repeated with pleaſure. Spirit of wine a little warmed, is commonly made uſe of for this purpoſe. The experiment will not fail to ſucceed, if a pretty ſtrong ſpark be drawn, in any manner, or direction whatever, through any part of it; and this may eaſily be done many ways, if it be contained in a metal ſpoon with a pretty wide mouth. A candle newly blown out may be lighted again by the electric ſpark paſſing through the groſs part of the ſmoke, within half an inch of the ſnuff; though it is perhaps blown in again by the motion given to the air by the force of the exploſion. Alſo air produced by the effervefcence of ſteel filings with oil of vitrol diluted with water, and many other ſubſtance, which throw out an inflammable vapour, may be kindled by it.

The ſtrong phoſphoreal or ſulphureous ſmell, which may be perceived by preſenting the noſtrils within an inch or two of any electrified point, makes a curious experiment, but it does not give a pleaſing ſenſation.

Lastly, the moſt entertaining experiment that can be performed by ſimple electrification, is when one or more of the company ſtand upon an inſulated ſtool, holding a chain from the prime conductor. In this caſe, the whole body

body is, in reality, a part of the prime conductor, and will exhibit all the ſame appearances, emitting ſparks wherever it is touched by any perſon ſtanding on the floor. If the prime conductor be very large, the ſparks may be too painful to be agreeable, but if the conductor be ſmall, the electrification moderate, and none of the company preſent touch the eyes, or the more tender parts of the face of the perſon electrified, the experiment is diverting enough to all parties.

MOST of the experiments above mentioned may alſo be performed to the moſt advantage by the perſon ſtanding upon the ſtool, if he hold in his hand whatever was directed to be faſtened to the prime conductor. Spirit of wine may be fired by a ſpark from a perſon's finger as effectually as in any other way. Care muſt be taken that the floor on which the ſtool is placed be free from duſt, but it is moſt adviſable to have a large ſmooth board for the purpoſe.

SECTION II.

ENTERTAINING EXPERIMENTS PERFORMED BY MEANS OF THE LEYDEN PHIAL.

NO electrical experiments anſwer the joint purpoſe of pleaſure and ſurprize in any manner comparable to thoſe that are made by means of the Leyden phial. All the varieties of electrical attraction and repulſion may be exhibited, either by the wire, or the coating of it; and if the knobs of two wires, one communicating with the inſide, and the other with the outſide of the phial, be brought within four or five inches of one another, the electrical ſpider above mentioned will dart from the one to the other in a very ſurpriſing manner, till the phial be diſcharged. But the peculiar advantage of the Leyden experiment is, that, by this means, the electrical flaſh, report, and ſenſation, with all their effects, may be increaſed to almoſt any degree that is deſired.

WHEN the phial, or the jar, is charged, the ſhock is given through a perſon's arms and breaſt, by directing him to hold a chain communicating with the outſide in one hand, and to touch the wire of the phial, or any conductor communicating with it, with the other

other hand. Or the ſhock may be made to paſs through any particular part of the body without much affecting the reſt, if that part, and no other, be brought into the circuit through which the fire muſt paſs from one ſide of the phial to the other.

A GREAT deal of diverſion is often occaſioned by giving a perſon a ſhock when he does not expect it; which may be done by concealing the wire that comes from the outſide of the phial under the carpet, and placing the wire which comes from the inſide in ſuch a manner in a perſon's way, that he can ſuſpect no harm from putting his hand upon it, at the ſame time that his feet are upon the other wire. This, and many other methods of giving a ſhock by ſurprize, may eaſily be executed by a little contrivance; but great care ſhould be taken that theſe ſhocks be not ſtrong, and that they be not given to all perſons promiſcuouſly.

WHEN a ſingle perſon receives the ſhock, the company is diverted at his ſole expence; but all contribute their ſhare to the entertainment, and all partake of it alike, when the whole company forms a circuit, by joining their hands; and when the operator directs the perſon who is at one extremity of the circuit to hold a chain which communicates with the coating, while the perſon who is at the other extremity of the circuit touches the wire. As all the perſons who form this circuit are ſtruck at the ſame time, and with the ſame degree of force, it is often very pleaſant to ſee them ſtart at the ſame moment, to hear them compare their ſenſations, and obſerve the very different accounts they give of it.

This experiment may be agreeably varied, if the operator, inſtead of making the company join hands, direct them to tread upon each others toes, or lay their hands upon each others heads; and if, in the latter caſe, the whole company ſhould be ſtruck to the ground, as it happened when Dr. Franklin once gave the ſhock to ſix very ſtout robuſt men, the inconvenience ariſing from it will be very inconſiderable. The company which the Doctor ſtruck in this manner neither heard nor felt the ſtroke, and immediately got up again, without knowing what had happened. This was done with two of his large jars (each containing about ſix gallons) not fully charged *.

The moſt pleaſing of all the ſurprizes that are given by the Leyden phial is that which Mr. Kinnerſley † contrived and called the MAGIC PICTURE, which he deſcribes in the following manner. Having a large metzotinto, with a frame and glaſs (ſuppoſe of the king) take out the print, and cut a pannel out of it, near two inches diſtant from the frame all round. If the cut be through the

* Franklin's Letters, new edition, p. 324.
† Ibid. p. 29.

picture, it is not the worse. With thin paste, or gum water, fix the board that is cut off on the inside of the glass, pressing it smooth and close: then fill up the vacancy, by gilding the glass well with leaf gold, or brass. Gild likewise the inner edge of the back of the frame all round, except the top part, and form a communication between that gilding and the gilding behind the glass; then put in the board, and that side is finished. Turn up the glass, and gild the foreside exactly over the back gilding; and when it is dry, cover it, by pasting on the pannel of the picture that has been cut out, observing to bring the correspondent parts of the board and picture together, by which the picture will appear of a piece as at first, only part is behind the glass, and part before. Lastly, hold the picture horizontally by the top, and place a little moveable gilt crown, on the king's head

If now the picture be moderately electrified, and another person take hold of the frame with one hand, so that his fingers touch its inside gilding, and with the other hand endeavour to take off the crown, he will receive a terrible blow, and fail in the attempt. The operator who holds the picture by the upper end, where the inside of the frame is not gilt, to prevent its falling, feels nothing of the shock, and may touch the face of the picture without danger, which he pretends to be a test of his loyalty. If a ring

ring of perſons take a ſhock among them, the experiment is called the CONSPIRATORS.

As the electric fire may be made to take whatever circuit the operator ſhall pleaſe to direct, it may be thrown into a great variety of beautiful forms. Thus, if a charged phial be placed at one extremity of the gilding of a book, and the diſcharge be made by a wire which touches the other extremity, the whole gilding will be rendered luminous. But if ſeveral pretty ſtrong ſhocks be ſent through the ſame gilding, they will ſoon render it incapable of tranſmitting any more, by breaking and ſeparating the parts too far aſunder. Alſo the electric conſtellations and figures, mentioned above, may be lighted up much more ſtrongly by a charged phial than by ſparks from the conductor; only, they cannot be lighted up ſo often in this way.

ON the ſame principle that the wires of phials charged differently will attract and repel differently, is made an ELECTRICAL WHEEL, which Dr. Franklin ſays, turns with conſiderable ſtrength, and of which he gives the following deſcription. A ſmall upright ſhaft of wood paſſes at right angles through a thin round board, of about twelve inches diameter, and turns on a ſharp point of iron, fixed in the lower end; while a ſtrong wire in the upper end, paſſing through a ſmall hole in a thin braſs plate, keeps the ſhaft truly vertical. About thirty *radii*, of equal length, made of faſh glaſs, cut in narrow ſlips, iſſue horizontally from the circumference of the board; the ends moſt diſtant from the center being about four inches apart. On the end of every one a braſs thimble is fixed.

If now the wire of a bottle, electrified in the common way, be brought near the circumference of this wheel, it will attract the neareſt thimble, and ſo put the wheel in motion. That thimble, in paſſing by, receives a ſpark, and thereby being electrified, is repelled, and ſo driven forwards, while a ſecond being attracted approaches the wire, receives a ſpark, and is driven after the firſt; and ſo on, till the wheel has gone once round; when the thimbles before electrified approaching the wire, inſtead of being attracted, as they were at firſt, are repelled, and the motion preſently ceaſes.

But if another bottle, which had been charged through the coating, be placed near the ſame wheel, its wire will attract the thimble repelled by the firſt, and thereby double the force that carries the wheel round; and not only take out the fire that had been communicated by the thimbles to the firſt bottle, but even robbing them of their natural quantity, inſtead of being repelled when they come again towards the firſt bottle, they are more ſtrongly attracted; ſo that the wheel mends its pace, till it goes with great rapidity, twelve or fifteen rounds in a minute, and with

with ſuch ſtrength, that the weight of one hundred Spaniſh dollars, with which we once loaded it, did not in the leaſt ſeem to retard its motion. This is called an ELECTRICAL JACK, and if a large fowl was ſpitted on the upper ſhaft, it would be carried round before a fire, with a motion fit for roaſting.

BUT this wheel, continues the Doctor, like thoſe driven by wind, moves by a foreign force, to wit that of the bottles.

THE SELF MOVING WHEEL, though conſtructed on the ſame principles, appears more ſurpriſing. It is made of a thin round plate of window glaſs, ſeventeen inches diameter, well gilt on both ſides, all but two inches next the edge. Two ſmall hemiſpheres of wood are then fixed with cement to the middle of the upper and under ſides, centrally oppoſite; and in each of them a thick ſtrong wire, eight or ten inches long, which together makes the axis of the wheel. It turns horizontally, on a point at the lower end of its axis, which reſts on a bit of braſs, cemented within a glaſs ſalt cellar. The upper end of its axis paſſes through a hole in a thin braſs plate, cemented to a long and ſtrong piece of glaſs; which keeps it ſix or eight inches diſtant from any non-electric, and has a ſmall ball of wax or metal on its top, to keep in the fire.

IN a circle on the table which ſupports the wheel, are fixed twelve ſmall pillars of glaſs, at at about eleven inches diftance, with a thimble on the top of each. On the edge of the wheel is a fmall leaden bullet, communicating by a wire with the gilding of the upper furface of the wheel; and about fix inches from it, is another bullet, communicating, in like manner, with the under furface. When the wheel is to be charged by the upper furface, a communication muft be made from the under furface to the table.

When it is well charged, it begins to move. The bullet neareft to a pillar moves towards the thimble on that pillar, and, paffing by, electrifies it, and then pufhes itfelf from it. The fucceeding bullet, which communicates with the other furface of the glafs, more ftrongly attracts that thimble, on account of its being electrified before by the other bullet, and thus the wheel increafes its motion, till the refiftance of the air regulates it. It will go half an hour, and make, one minute with another, twenty turns in a minute, which is 600 turns in the whole, the bullet of the upper furface giving in each turn twelve fparks to the thimbles, which makes 7200 fparks, and the bullet of the under furface receiving as many from the thimble, thofe bullets moving in the time near 2500 feet. The thimbles are well fixed, and in fo exact a circle, that the bullets may pafs within a very fmall diftance of each of them.

If inftead of two bullets, you put eight, four communicating with the upper furface, and

and four with the under ſurface, placed alternately (which eight, at about ſix inches diſtance, complete the circumference) the force and ſwiftneſs will be greatly increaſed, the wheel making fifty turns in a minute, but but then it will not continue moving ſo long.

These wheels, the Doctor adds, may be applied perhaps to the ringing of chimes, and moving light made orreries *.

A phial makes the moſt beautiful appearances when it is charged without any coating on the outſide, by putting the hand, or any conductor, to it: for then, at whatever part of the jar the diſcharge is made, the fire will be ſeen to branch from it in moſt beautiful ramifications all over the jar, and the light will be ſo intenſe, that the minuteſt of the branches may be ſeen in open day-light.

The diſcharge of a large electrical battery is rather an awful than a pleaſing experiment, and the effects of it, in rending various bodies, in firing gun-powder, in melting wires, and in imitating all the effects of lightning, never fail to be viewed with aſtoniſhment. In order to fire gun-powder, it muſt be made up into a ſmall cartridge, with blunt wires inſerted at each end, and brought within half an inch of each other, through which the ſhock muſt paſs: or a very ſmall wire may be drawn through the center of it, and the ex-

* Franklin's Letters, p. 28, &c.

plosion will be made by its melting. A common jar will easily strike a hole through a thick cover of a book, or many leaves of paper, and it is curious to observe the bur raised on both sides, as if the fire had darted both ways from the center.

A CONSIDERABLE number of experiments with an electrical battery some of which exhibit fine appearances, will be particularly described in the last part of the work.

SECTION

SECTION III.

Entertaining experiments made by a combination of philosophical instruments.

In order to exhibit ſome of the fineſt electrical experiments, the operator muſt call to his aid other philoſophical inſtruments, particularly the condenſing machine, and the air pump.

If the fountain made by condenſed air be inſulated, and be made to emit one ſtream, that ſtream will be broken into a thouſand, and equally diſperſed over a great ſpace of ground, when the fountain is electrified; and by only laying a finger upon the conductor, and taking it off again, the operator may command either the ſingle ſtream, or the divided ſtream at pleaſure. In the dark, the electrified ſtream appears quite luminous.

The greateſt quantity of electric light is ſeen *in vacuo*. Take a tall receiver very dry, and in the top of it inſert with cement a wire not very acutely pointed. Then exhauſt the receiver, and preſent the knob of the wire to the conductor, and every ſpark will paſs through the vacuum in a broad ſtream of light, viſible through the whole length of the receiver, be it ever ſo tall. This ſtream often divides

divides itſelf into a variety of beautiful rivulets, which are continually changing their courſe, uniting and dividing again, in a moſt pleaſing manner. If a jar be diſcharged through this vacuum, it gives the appearance of a very denſe body of fire, darting directly through the center of the vacuum, without ever touching the ſides; whereas, when a ſingle ſpark paſſes through, it generally goes more or leſs to the ſide, and a finger put on the outſide of the glaſs will draw it wherever a perſon pleaſes. If the veſſel be graſped by both hands, every ſpark is felt like the pulſation of a great artery, and all the fire makes towards the hands. This pulſation is felt at ſome diſtance from the receiver; and in the dark, a light is ſeen betwixt the hands and the glaſs.

ALL this while the pointed wire is ſuppoſed to be electrified poſitively; if it be electrified negatively, the appearance is remarkably different. Inſtead of ſtreams of fire, nothing is ſeen but one uniform luminous appearance, like a white cloud, or the milky way in a clear ſtar-light night. It ſeldom reaches the whole length of the veſſel, but is generally only like a lucid ball at the end of the wire.

A VERY beautiful appearance of electric light in a darkened room may alſo be produced by inſerting a ſmall phial into the neck of a tall receiver, ſo that the external ſurface of the glaſs may be expoſed to the vacuum. The phial muſt be coated on the inſide, and whil it is charging, at every ſpark taken from the conductor into the inſide, a flaſh of light is ſeen to dart, at the ſame time, from every part of the external ſurface of the jar, ſo as quite to fill the receiver. Upon making the diſcharge, the light is ſeen to return in a much cloſer body, the whole coming at once.

But the moſt beautiful of all the experiments that can be exhibited by the electric light is Mr. Canton's AURORA BOREALIS, of which the following is but an imperfect deſcription. Make a Torricellian vacuum in a glaſs tube, about three feet long, and ſeal it hermetically, whereby it will be always ready for uſe. Let one end of this tube be held in the hand, and the other applied to the conductor, and immediately the whole tube will be illuminated, from end to end; and when taken from the conductor, will continue luminous without interruption for a conſiderable time, very often above a quarter of an hour. If, after this, it be drawn through the hand either way, the light will be uncommonly intenſe, and without the leaſt interruption from one hand to the other, even to its whole length. After this operation, which diſcharges it in a great meaſure, it will ſtill flaſh at intervals, though it be held only at one extremity, and quite ſtill; but if it be graſped by the other hand, at the ſame time, in a different place,

place, ſtrong flaſhes of light will hardly ever fail to dart from one end to the other; and this will continue twenty four hours, and perhaps much longer, without freſh excitation. Small and long glaſs tubes exhauſted of air, and bent in many irregular crooks and angles, will, when properly electrified in the dark, beautifully repreſent flaſhes of lightning.

I SHALL conclude this deſcription of entertaining experiments with an account of the manner in which Dr. Franklin and his friends cloſed the year 1748. The hot weather coming on, when electrical experiments were not ſo agreeable, they put an end to them for that ſeaſon, as the Doctor ſays, ſomewhat humorouſly, in a party of pleaſure on the banks of the Skuylkil. Firſt, ſpirits were fired by a ſpark ſent from ſide to ſide through the river, without any other conductor than the water. A turkey was killed for their dinner by the electrical ſhock, and roaſted by the electrical jack, before a fire kindled by the electrified bottle, when the healths of all the famous electricians in England, Holland, France, and Germany, were drunk in electrified bumpers, under a diſcharge of guns from the electrical battery *.

HAPPY would the author of this treatiſe be to ſee all the great electricians of Europe,

* Franklin's Letters, p. 35.

or even thofe in England, upon fuch an occafion, and efpecially after having made difcoveries in electricity of equal importance with thofe made in Philadelphia in the year referred to. With pleafure would he obey a fummons to fuch a rendezvous, though it were to ferve the illuftrious company in the capacity of operator, or even in the more humble office of waiter. Chearfulnefs and focial intercourfe do, both of them, admirably fuit, and promote the true fpirit of philofophy.

PART

PART VIII.

NEW EXPERIMENTS IN ELECTRICITY, MADE CHIEFLY IN THE YEAR 1766.

I SHALL, in the laſt part of this work, preſent my reader with an account of ſuch new experiments in electricity as this undertaking has led me to make. I hope the peruſal of this work may ſuggeſt many more, and more conſiderable ones to my readers, and then I ſhall not think that I written in vain.

To make this account the more uſeful to ſuch perſons as may be willing to enter into philoſophical inveſtigations, I ſhall not fail to report the real views with which every experiment was made, falſe and imperfect as they often were. I was always greatly pleaſed with the extreme exactneſs and ſimplicity of Mr. Grey, and ſhall, therefore, imitate his artleſs manner. And though an account of experiments drawn up on this plan be leſs calculated to do an author honour as a philoſopher; it will, probably, contribute more to make other perſons philoſophers, which is a thing of much more conſequence to the public.

Many modeſt and ingenious perſons may be engaged to attempt philoſophical inveſtigations, when they ſee, that it requires no more ſagacity to find new truths, than they themſelves are maſters of; and when they ſee that many diſcoveries have been made by mere accident, which may prove as favourable to them as to others. Whereas it is a great diſcouragement to young and enterpriſing geniuſes, to ſee philoſophers propoſing that firſt, which they themſelves attained to laſt; firſt laying down the propoſitions which were the reſult of all their experiments, and then relating the facts, as if every thing had been done to verify a true preconceived theory.

This *ſynthetic* method is, certainly, the moſt expeditious way of making a perſon underſtand a branch of ſcience; but the *analytic* method, in which diſcoveries were actually made, is moſt favourable to the progreſs of knowledge.

I have, indeed, endeavoured to make the whole preceding hiſtory of electricity uſeful in this view, by not contenting myſelf with informing the reader what diſcoveries have been made; but, wherever it could be done, acquainting him *how* they were made, and what the authors of them had in view when they made them. In general, this has not been difficult to do, the facts being recent, and moſt of the perſons concerned now living. And, perhaps, in no branch of ſcience

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has there been lefs owing to genius, and more to accident; fo that no perfon, who will give a little attention to the fubject, need be without hopes of adding fomething to the common ftock of electrical difcoveries. Nay, it would be extraordinary, if, in a great number of experiments, in which things were put into a variety of new fituations, no new fact, worth communicating to the public, fhould arife.

THE method I propofe will, likewife, give the moft pleafure to thofe perfons, who delight in tracing the real progrefs of the human mind, in the inveftigation of truth, and the acquifition of knowledge; as I hope it will carry with it fufficient evidence of its own authenticity. For this progrefs, we may affure ourfelves, has, in all cafes, been by eafy fteps, even when it has been the moft rapid. Were it poffible to trace the fucceffion of ideas in the mind of Sir Ifaac Newton, during the time that he made his greateft difcoveries, I make no doubt but our amazement at the extent of his genius would a little fubfide. But if, when a man publifhes difcoveries, he, either through defign, or through habit, omit the intermediate fteps by which he himfelf arrived at them; it is no wonder that his fpeculations confound others, and that the generality of mankind ftand amazed at his reach of thought. If a man afcend to the top of a building by the help of a common ladder, but cut away moft of the fteps after he has done with them, leaving only every ninth or tenth ſtep; the view of the ladder, in the condition in which he has been pleaſed to exhibit it, gives us a prodigious, but an unjuſt idea of the man who could have made uſe of it. But if he had intended that any body ſhould follow him, he ſhould have left the ladder as he conſtructed it, or perhaps as he found it, for it might have been a mere accident that threw it in his way. It is poſſible he had even better have deſtroyed it intirely; as, in ſome caſes, a perſon would more eaſily make a new ladder of his own, than repair an old and damaged one.

THAT Sir Iſaac Newton himſelf owed ſomething to a caſual turn of thought, the hiſtory of his aſtronomical diſcoveries informs us; and where we ſee him moſt in the character of an experimental philoſopher, as in his optical inquiries (though the method of his treatiſe on that ſubject is by no means purely analytical) we may eaſily conceive that many perſons, of equal patience and induſtry (which are not called qualities of the underſtanding) might have done what he did. And were it poſſible to ſee in what manner he was firſt led to thoſe ſpeculations, the very ſteps by which he purſued them, the time that he ſpent in making experiments, and all the unſucceſsful and inſignificant ones that he made in the courſe of them; as our pleaſure of one kind would be increaſed, our admiration would probably decreaſe. Indeed he himſelf uſed candidly to acknowledge, that if he had done more than other men, it was owing rather to a habit of *patient thinking*, than to any thing elſe.

I do not ſay theſe things to detract from the merit of the great Sir Iſaac Newton; but I think that the intereſts of ſcience have ſuffered by the exceſſive admiration and wonder, with which ſeveral firſt-rate philoſophers are conſidered; and that an opinion of the greater equality of mankind, in point of genius, and powers of underſtanding, would be of real ſervice in the preſent age. It would bring more labourers into the common field; and ſomething more, at leaſt, would certainly be done in conſequence of it. For though I by no means think that philoſophical diſcoveries are at a ſtand, I think the progreſs might be quickened, if ſtudious and modeſt perſons, inſtead of confining themſelves to the diſcoveries of others, could be brought to entertain the idea, that it was poſſible to make diſcoveries themſelves. And, perhaps, nothing would tend more effectually to introduce that idea, which is at preſent very remote from the minds of many, in which it ought to have a place, than a faithful hiſtory of the manner in which philoſophical diſcoveries have actually been made by others.

That this fidelity has been preſerved in the following narrative, I make no doubt of its being its own voucher. Its imperfections will be a ſufficient evidence. The ſame fidelity will alſo oblige me to relate ſeveral facts as appearing new to myſelf, which the courſe of the preceding hiſtory will ſhow to have been

been difcovered by others, though I was not then aware of it. Of fuch after-difcoveries, however, I have mentioned only thofe which, it will be feen, I have purfued fomething farther than the original authors, having attended to circumftances overlooked by them; or, at leaft, having made the experiments with more exactnefs, fo that the reader may expect fomething really new under every article. And the experiments which prove the fame thing will be found confiderably different from thofe of others, and to furnifh additional arguments of the fame general propofitions. This repetition of old difcoveries, and this variety in the experiments by which they were made, were both occafioned by a fituation which is more or lefs common to every electrician in England; whereby we are ignorant of a great deal of what has been done by others.

In the following narrative will alfo be found an account not only of experiments which are complete, which exhibit fome new fact, and from which fomething relating to the general theory of electricity may be deduced; but alfo fome that are incomplete, which produced no new appearance, and from which nothing pofitive could be concluded. If electricians in general had done this, they would have faved one another a great deal of ufelefs labour, and would have had more time for making experiments really new, and which might have terminated in confiderable difcoveries. Befides, if things be really put into new fituations, though nothing *pofitive* can be inferred from

from the experiment, at leaſt ſomething negative may; and this cannot be ſaid to be of no importance in ſcience; nor, ſtrictly ſpeaking, to be no new truth. A ſufficient number of theſe experiments may, in many caſes, lay a foundation for probable and poſitive concluſions.

I MAKE no apology for leaving ſo many of theſe experiments imperfect, and for publiſhing this account of them before they have been purſued ſo far as it may, perhaps, be thought they deſerve. I rather think the generality of philoſophers ought to make an apology to the public, for delaying the communication of their experiments and diſcoveries ſo long as they have done. It is poſſible I may never have any more leiſure or opportunity to purſue them, and others may better command both; whereby the diſcoveries will be ſooner brought to their maturity, and the progreſs of this branch of philoſophy accelerated. The genuine ſpirit of philoſophy is, ſurely, not that of mechanics, who make the moſt of every little improvement in their arts, and never divulge them, till they can make no more advantage of them themſelves. If I could this day communicate to any fellow-labourer a hint, which it was more probable he could immediately purſue to advantage than myſelf, I would not defer it till to-morrow. Nor do I think it is any great boaſt of a philoſophical indifference to fame to make this declaration. The great Sir Iſaac Newton ſeems to have had no idea of the purſuit of

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fame. He deferred the communication of his important diſcoveries through real modeſty, thinking it impertinent to trouble the public with any thing imperfect. I make no pretenſions to that kind of modeſty. Whether it be of a true or a falſe kind, I think it manifeſtly injurious to the progreſs of knowledge. Like thoſe who contended in one of the games of ancient Greece, I ſhall immediately deliver my torch to any perſon who can carry it with more dexterity. If others do the ſame, it may come into my hands again, ſeveral times, before we reach the goal.

It may be ſaid, that I ought, at leaſt, to have waited till I had ſeen the connection of my new experiments with thoſe that were made before, and have ſhown that they were agreeable to ſome general theory of electricity. But when the facts are before the public, others are as capable of ſhowing that connection, and of deducing a general theory from them as myſelf. If but the moſt inconſiderable part, of the temple of ſcience be well laid out, or a ſingle ſtone proper for, and belonging to it be collected; though at preſent it be ever ſo much detached from the reſt of the building, its connection and relative importance will appear in due time, when the intermediate parts ſhall be completed. Every *fact* has a real, though unſeen connection with every other fact: and when all the facts belonging to any branch of ſcience are collected, the ſyſtem will form itſelf. In the mean time, our gueſſing at the ſyſtem may be ſome guide

guide to us in the difcovery of the facts; but, at prefent, let us pay no attention to the fyftem in any other view; and let us mutually communicate every new fact we difcover, without troubling ourfelves about the fyftem to which it may be reduced.

I THINK I fhall give the moft diftinct view of the few things that I have obferved, in the fhort courfe of my electrical experiments, if I relate them pretty nearly in the order in which they occurred, only taking care not to intermix things of a very different nature. The earlieft date in my experiments is the beginning of the year 1766; when, in confequence of forming an acquaintance with fome gentlemen who have diftinguifhed themfelves for their difcoveries in electricity, and of undertaking to write the preceding hiftory, my attention was firft turned towards making fome original experiments in this part of Natural Philofophy, which had ferved for my occafional amufement fome time before.

SECTION I.

EXPERIMENTS ON EXCITATION, PARTICULARLY OF TUBES IN WHICH AIR IS CONDENSED; AND OF LARGE GLASS GLOBES.

FINDING by my own experiments, and thoſe of others, that a glaſs tube, out of which the air was exhauſted, diſcovered no ſign of electricity outwards, but that all its effects were obſerved on the inſide; I imagined that, if the air was condenſed in the tube, it would operate more ſtrongly on the outſide; ſo that an additional atmoſphere would give it a double virtue. But the reſult was the very reverſe of my expectations.

SOME time in the month of January, when the weather was dry and froſty, I took a glaſs tube, ſuch as is generally uſed for electrifying, about two feet and a half in length, and an inch in diameter. It was cloſed at one end, and by means of a braſs cap at the other, I fitted a condenſing engine to it; and when the tube was very dry, and in excellent order for making experiments, I began to throw in more air. At every ſtroke of the piſton I endeavoured to excite the tube, but found its virtue diminiſhed. It was obliged

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to be brought nearer than before to attract light bodies, and gave lefs light when rubbed in the dark; till, as near as I could judge, I had got one additional atmofphere into the tube, when its power was fcarce difcernible. Letting out the air by degrees, I obferved it gradually recovered its power. It attracted light bodies at a greater diftance; it gave louder fnappings, and more light in the dark; and when the additional air was wholly let out, its power was immediately as great as it had been before any air was thrown it. This I tried feveral times with the fame fuccefs.

Communicating thefe experiments to Dr. Franklin and Dr. Watfon, they fuggefted to me, that the non-excitation of the tube above mentioned might be owing to moifture introduced along with the air, and adhering to the infide of the tube. This conjecture was rendered more probable by another experiment I had made in the mean time.

Repeating my attempts to excite the tube above mentioned, I found that, after very hard rubbing, it began to act a little; and that its virtue encreafed with the labour. Thinking it might be the warmth which produced this effect, I held the tube to the fire, and found that when it was pretty hot, it would act almoft as well as when it contained no more than its ufual quantity of air. I conjectured that the warmth might expel the moifture from the fides of the glafs, or make the enclofed

enclofed air capable of holding a greater quantity of water in a ftate of perfect folution.

WILLING to determine whether the additional quantity of air with the moifture it occafioned, acted in all refpects like a non-electric coating, I tried the experiment with condenfed air, that Dr. Defaguliers did with fand. After condenfing the air, and finding the excitation of it impoffible, as ufual, I let the air fuddenly out, to fee whether the tube would then fhow any effect of the preceding friction; but it had not acquired the fmalleft degree of electricity; though the firft ftroke of the rubber, immediately afterwards, made it give fparks to the finger at the diftance of two and three inches. Perhaps the degree of moifture it had contracted was very flight, and expelled by the act of excitation.

UPON being defired to repeat this experiment with a particular view to the moifture; I obferved that not the leaft cloudinefs could be perceived to adhere to the glafs, at the time that it was abfolutely incapable of excitation. When one part of the tube was made warm, and the other left cold, the fame ftroke of the rubber would excite the warm part, without in the leaft affecting the reft. But ftill the cold part of the tube appeared not in the leaft more cloudy than that which was warm; and the moment the air was let out, the firft ftroke of the rubber made the whole ftrongly electrical.

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Willing to aſcertain whether the condenſing of air neceſſarily introduced more moiſture into a glaſs veſſel than the air could hold in perfect ſolution, I conſtructed a glaſs condenſer in ſuch a manner, that I could charge and diſcharge ſmall phials in the inſide of it; concluding, that if the additional air brought more additional moiſture, it would be impoſſible to charge a phial at all in thoſe circumſtances; whereas, if the air was free from moiſture, it would make the phial hold a greater charge, double in two atmoſpheres, treble in three, &c. Accordingly, I charged a tube about three quarters of an inch in diameter, and coated about eight inches, in the glaſs veſſel, containing about two atmoſpheres; and it received a much greater charge than it could be made to take in the open air, and as near as could be judged, by the report and flaſh, twice as great. At laſt the tube burſt by a ſpontaneous diſcharge, after being charged and diſcharged three or four times, in the condenſed air. It is not at all probable, that it could have been broke by any charge it could have held in the open air. This experiment ſeemed to determine, that there was no very great degree of moiſture introduced into the glaſs veſſel by the condenſation of air.

I afterwards found that experiments on condenſed air had been made by Mr. Du Fay and others, but not with all the circumſtances above mentioned.

SOME of my electrical friends are of opinion, that the reaſon why a tube with condenſed air in it cannot be excited is, that the denſe air within prevents the electric fluid from being forced out of the inſide of the tube, without which none can be forced into the outſide; and that heating the tube makes the air within leſs electrical, and the tube alſo; in conſequence of which, it may more eaſily part with the fluid on one ſide, and admits it on the other. But upon this principle how can a ſolid ſtick of glaſs be excited?

IMAGINING that a greater quantity of electric fire would be produced from the friction of larger globes than thoſe of the uſual ſize, I provided myſelf on the 24th of April 1766, with a globe ſeventeen inches and a half in diameter. It had only one neck, and was made exceedingly well; only being rather too large for the mouth of the furnace, a ſmall coal had ſtuck to its equatorial diameter, which, when it was ſtruck off, made a ſmall hole in it. This, in ſome meaſure, disfigured the globe, but I never imagined it could prevent its excitation in any great degree; ſo that I ſtill indulged hopes of acquiring, by its means, a prodigious power of electricity. But what was my ſurprize when, after I had got it mounted in the beſt manner poſſible, and after trying, for hours together, every method of friction, in the moſt favourable circumſtances for excitation, I could ſcarce get the appearance of fire from it; the ſparks from the prime conductor being barely visible.

Acquainting Dr. Franklin with my disappointment, he advised me to get the first coat of the globe taken off with emery; as it had often been observed, that many globes would not work well, till after a considerable time, when the glass-house coat, as it may be called, is worn off. This operation I accordingly performed upon it, and incredibly laborious it proved; which greatly increased my disappointment, when I found that it had all been labour in vain, for the globe had no more electrical power than before.

Dispairing of making any thing of this globe, I laid it aside, and, on the 22d of May, got another, about fourteen inches in diameter. In blowing this globe, every circumstance that I could imagine had, in the least, contributed to my ill-success with the former, was carefully avoided. The former was made late in the week, when the metal had been long in fusion; because I had been told, that globes made in that state of the metal were always the best for electrical purposes. This was blown early in the week, when the workmen say the metal is most transparent, and freest from all kinds of imperfections. The former was warmed, in the course of making it, in a place in which wood and coals were frequently thrown, to keep up the heat. This was kept free from the fumes of any fuel whatever. Nothing could be finer than the metal of this globe, nothing more perfect in its form. It was alſo very well mounted, and I did not doubt of ſucceſs. But, after all, this globe, if poſſible, gave leſs fire than the former. I had recourſe to every method of excitation that I had ever heard of, or could myſelf imagine, but all in vain. The thing looked like enchantment.

Whilst I was thinking over every thing that I could imagine might poſſibly be the the cauſe of my ill ſucceſs with theſe globes, I recollected, that another globe, which I had got made for a friend, in the ſame ſtate of the metal with my laſt, and only an inch and half leſs in diameter, acted exceedingly well; and that there was no other apparent difference between them, but that his had two necks, and an axis quite through it; whereas mine had only one neck, and no axis at all. Willing to try every thing, I reſolved to get the braſs cap of my globe perforated, and a ſmall wire introduced, to ſerve inſtead of an axis. This was done; but, in making the perforation, it happened, unfortunately, as I then thought, but the moſt fortunately in the world as it proved, that a lump of hard cement, about the bigneſs of a ſmall walnut was puſhed into the inſide of the globe. Vexatious as this circumſtance was, I was impatient to try my new experiment, and immediately began to whirl the globe, with this ſuccedaneum of an axis, though the cement was all the while rattling, and fouling the inſide.

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I HAD not whirled the globe long, in theſe circumſtances, before I plainly perceived that its power increaſed. After ſome time it was pretty conſiderable, and I did not doubt but it was owing to the axis; nay I had formed a pretty plauſible theory, to account for an axis being neceſſary to a globe of ſuch a ſize. Willing, however, to verify the fact, and aſcertain my new hypotheſis, I took out the wire; but, to my ſurpriſe, found the virtue of the globe not at all diminiſhed. On the contrary, it continued increaſing, and by the time that the cement was well broken, and diſperſed, ſo as to have given a kind of lining to the globe, it power was exceedingly ſtrong, and it acted as well as any globe I had ever ſeen. In this ſtate, I obſerved, that after exciting any part of the ſurface, the ſmall pieces of cement in the inſide, to the diſtance of about two inches, would jump from the finger, or any conductor, preſented on the outſide.

HAVING, in this unexpected manner, made a perfect cure of this ſmaller globe, I remounted the larger, and conſidering, that the cement could probably act only as any other electric lining, I introduced into it ſome pounded ſulphur, mixed with ſome flower of brimſtone; and found that, as ſoon as there was enough to render it ſemi-opaque, it acted very well.

IN this ſtate, the appearance of the globe was, in ſeveral reſpects, very remarkable. The part that was rubbed had none of the ſulphur upon it, except thoſe places where the poliſh had been, in ſome meaſure, taken off by the emery, in the firſt operation. Theſe being circular, the brimſtone lay upon them, like the belts of Jupiter. The hemiſphere oppoſite to the neck had twice as much ſulphur upon it as the other; and, in both hemiſpheres, the ſulphur lay thicker, as it receded from the equatorial diameter.

I AFTERWARDS put as much more ſulphur into it, which doubled the lining equally every where; but left two or three great heaps, in particular parts of the equatorial diameter, where it was rubbed, and where I could perceive no defect of poliſh. Whirling the globe, upon this, I found the virtue almoſt quite gone, and even the amalgam could not revive it. Endeavouring to take the ſulphu out of the globe, I broke a great hole into it; and alſo the new globe was broken the ſame day, by a lump of hard cement, in the inſide, falling from the top to the bottom. Theſe accidents rendered my experiments incomplete.

I THEN propoſed to get another large globe, with one neck, and a large hole in the oppo ſite ſide; by means of which I could eaſily put different ſubſtances into it, and take them ou again, in order to find the cauſe of the appearances above mentioned. But apprehending this courſe of experiments might prove a litt. too expenſive, and after all, terminate in nothing, I unwillingly deſiſted.

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I SHALL add to this ſection, on the ſubject of excitation, that I once whirled a very thin globe, about ſix or ſeven inches in diameter, which was made to weigh air, and not one fourth part ſo thick as a common Florence flaſk. It was excited very powerfully by a piece of leather which had been ſoaked in a mixture of tallow and bees wax, and into which a quantity of amalgam had been worked. With this globe I could make my common jar diſcharge itſelf over more than five inches of the external ſurface, which I reckon to be a conſiderable proof of its power. It ſeems to follow from this experiment, that the thinneſs of glaſs globes, or tubes, is by no means any obſtruction to their electric power.

IN the courſe of theſe experiments I had read Mr. Bergman's account of his curing a globe by a lining of melted ſulphur, and had propoſed to try that in the laſt place, on account of the diſagreeable operation; but found it ſuperſeded in the manner deſcribed above.

SECTION II.

EXPERIMENTS WHICH PROVE A CURRENT OF AIR FROM THE POINTS OF BODIES ELECTRIFIED EITHER POSITIVELY OR NEGATIVELY.

DURING a courſe of electrical experiments, made to divert ſome of my friends, one of the company happened to preſent a pointed wire to my hand, as I was ſtanding upon an inſulated ſtool; when I was ſurpriſed to perceive a cool blaſt proceeding from it; though, according to Dr. Franklin's theory, the current of the fluid went from my hand to the point. I then preſented my noſtrils to the point, and perceived the ſame ſtrong phoſphoreal ſmell, as if the point had been electrified poſitively. Theſe facts made me entertain ſome doubts about the direction of the current, and the principles of Dr. Franklin's theory, and led me to the following courſe of experiments; which prove nothing againſt that theory, but eſtabliſh a real current of air from the points of all electrified bodies.

CONSIDERING that flame is the leaſt ſenſibly affected with electrical attraction or repulſion, but moſt eaſily with the leaſt breath of air; and not doubting at that time, but that the current

current of air would be in the direction of the fluid, being, as it were, impelled by it; I presented the flame of a candle to a pointed wire, electrified negatively, as well as positively. The blast was so strong (in both cases alike) as to lay bare the greatest part of the wick, the flame being driven from the point; and sometimes a pretty large candle would be actually blown out by the blast. But, in all cases, the effect was the same whether the electric fluid issued out of the point, or entered it.

PLACING the flame between two points, one of which communicated with the prime conductor electrified positively, and the other with the floor, the flame was blown from that which communicated with the conductor upon the other, but not to so great a distance as if the other had been away. Changing the points, the effect was still the same, whether that which communicated with the conductor was the more sharp, or the more blunt of the two, the flame always receding from it.

REVERSING this experiment, and making one of the points communicate with the rubber, and the other with the floor, the flame was always blown from the former towards the latter. It was evident, however, that the point which communicated with the floor had a current of air blowing from it likewise; for it counteracted the other, and would, when brought near the flame, raise it almost perpendicular, when it had been blown quite aside by the other.

Placing the flame between two points, one of which communicated with the rubber, and the other with the conductor, it was equally affected by both, being always blown from the point which was nearest to it.

It was very observable, that, notwithstanding the current of air from the points affected the flame so remarkably; yet a small portion of it, when it was brought very near the point, would be strongly attracted by it, at the same time that the greatest part of the flame was, by the current of air, blown the contrary way. This effect was always the same, whether the point was electrified positively or negatively; though, I fancied that the negative point attracted the flame more sensibly than the other.

Afterwards I diversified this experiment in the following manner. I charged the inside of a small jar positively, then setting it upon a glass stand, in contact with a pointed wire, I placed the flame of a candle within an inch of the point, and touched the wire of the jar, with a brass rod which I held in my hand. At every touch the flame was blown strongly from the point. Sometimes it would be blown out; but another point being held opposite to it, would support the flame; and more strongly, if that point was joined with the rod with which I touched the wire of the

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the jar. Charging the jar negatively in the inſide, all the effects were the very ſame. Diſcharging the jar through the points, with the flame in a right line between them, it was diſturbed, but not blown to one ſide more than the other.

To take off all the effect of the electrical attraction and repulſion, and leave the current of air to act ſingly, I interpoſed pieces of braſs wire communicating with the earth, between the points of the wire and the flame; and found the blaſt to be rather increaſed than diminiſhed thereby.

Having communicated theſe experiments to Dr. Franklin, he adviſed me to try the force of this current upon *paper vanes*, ſuch as he has deſcribed in his letters: for, with him, they ſeemed to turn one way or the other indifferently, juſt as they happened to ſet out. Accordingly I took a cork, and ſtuck into the ſides of it thirteen vanes, each being half a card, well dried, and each proceeding from the center of the cork. Into the cork I ſtuck a needle, by which I ſuſpended the whole on a magnet.

These vanes I held two or three inches from the point of a wire, communicating with the outſide coating of the jar, placed upon an electric ſtand, in the manner deſcribed above; and obſerved, that whenever I took a ſpark from the wire communicating with the inſide, the vanes were ſtrongly blown upon, and made to turn, as if the current of air had flowed from the point; at the

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ſame time that, according to Dr. Franklin's theory, the electric fluid was entering it. If they were made to turn the contrary way, the current ſoon ſtopped them, and never failed to bring them back, and make them move as before.

WHEN wires communicating with the floor were placed between the vanes and the point, to take off all the electrical attraction and repulſion, the vanes ſtill moved as briſkly as ever.

WHEN the jar was charged pretty high, the motion might be made ſo ſwift, that the ſeparate vanes could hardly be diſtinguiſhed, as the whole ſet turned round.

I MOREOVER obſerved, that the vanes were turned very briſkly, not only when held near the point, but alſo when held any where within the diſtance of ſix or ſeven inches from the ſides of the wire, which I made ſometimes of a conſiderable length. The ſtream would turn the vanes one way on one ſide of the wire, and the contrary way on the other; and being removed quickly to the different ſides, the direction of their courſe might be changed ſeveral times, in the diſcharge of one ſmall jar.

I MADE points to project two ways at the ſame time, and obſerved, that the ſtream was the ſame from both, and alſo when the points were made to project at right angles from one another. In this poſition of the wires, it was amuſing to obſerve, that the vanes would move one way, when held near one of the wires;

wires; and immediately turn about and move the contrary way, if removed near the other.

Hitherto I had made my vanes of very dry paper, in order to make them leſs affected by electrical attraction and repulſion, that ſo the current of air might be the more indiſputable; but Mr. Canton deſiring me to try vanes that were conductors, I firſt dipped my paper vanes in water, and afterwards made a ſet of tinſel, or thin pieces of braſs, of the ſame form with the other. Theſe vanes, being conductors of electricity, promoted a freer current of the electric matter, and conſequently, occaſioning a greater motion to be given to the air, they whirled about with more rapidity than the former. When they were inſulated, they were affected juſt as the dry paper vanes had been.

With theſe vanes, I diverſified the experiment in a manner which ſhowed the ſameneſs of the current, notwithſtanding the change of electricity, in a clearer manner than before. I inſulated a jar, with a wire projecting from the coating, and held the tinſel vanes near the extremity of it. All the time the jar was charging, the vanes turned with great rapidity, as if by a blaſt from the point. Keeping the jar, the pointed wire, and the vanes in the ſame ſituation, the gradual diſcharge of the jar, made by now and then touching the wire which communicated with the inſide, made the vanes ſtill turn the ſame way, and, as far as could be perceived, with the ſame force.

To

To diverſify this experiment, I placed a charged jar upon a ſtool which had glaſs feet, a pointed wire projecting from the coating, a quantity of braſs duſt before the point, and a braſs chain communicating with the ground on the other ſide of the duſt. In this ſituation every attempt to diſcharge the jar threw a conſiderable quantity of the duſt from the point, being raiſed about ſeven or eight inches, and blown to a conſiderable diſtance. Removing the pointed wire from the coating of the jar, and connecting it with the chain, the ſame attempt to diſcharge it blew the duſt upon the jar. Uſing two points, one at the jar, and the other at the chain, the duſt was diſturbed, and raiſed up, but not blown one way more than the other. Fine flour anſwered nearly as well.

Lastly, I made the experiment of the current with vanes in the form of a ſmoke jack, which anſwered as well as the others. They were moved when held more than a foot above the point, and likewiſe at a conſiderable diſtance below it, when it was turned downwards.

After theſe experiments, I read in Mr. Wilſon's treatiſe on electricity, that the vanes would not turn in vacuo. This I tried, and found it to be true, and at the ſame time I found, they would not turn in a cloſe receiver, not exhauſted, where the air was confined, and had not a free circulation.

The current of air from the points of bodies electrified *plus* or *minus*, is not more dif-

ficult

ficult to be account for on Dr. Franklin's hypothesis of positive and negative electricity, than any other case of electrical repulsion. The particles of the atmosphere, near the points of electrified bodies, having, by their means, become possessed of more or less than their natural share of the electric fluid, must, according to the rule above mentioned, retire to places where they can discharge or replenish themselves, as occasion may require. If it be asked why the particles of the atmosphere do not, in the same manner, recede from all the parts of the electrified body, as well as from the points; it is answered, that, as the pressure of the atmosphere will prevent a vacuum, and as electrical attraction and repulsion are most powerful at the points of bodies, on account of the easier entrance or exit of the fluid at the points (upon whatever principle that effect depends) the electrified atmosphere (whether negative or positive makes no difference) must fly off at the points preferably to any other places, and the weight of the atmosphere will force the air of the neighbouring places upon the flatter parts of the electrified conductor, notwithstanding the real endeavour it may have to recede from it.

SECTION III.

Experiments on fixed AIR, and CHARCOAL.

DR. Franklin, to whom I had communicated ſome imperfect experiments on the electricity of noxious air, recollected them when he was this laſt ſummer at Pyrmont, where a large body of fixed air always lies upon the ſurface of the medicinal ſpring (for this air is evidently ſpecifically heavier than common air, and does not eaſily mix with it) but not having a proper apparatus, and the company there making experiments inconvenient, he did nothing that was deciſive; though, from the little that he had an opportunity of doing, he imagined that it was not a conductor: and I have ſince found that this ſuppoſition was juſt. A charged phial may be dipped into a body of fixed air, reſting on the ſurface of a fermenting vat, without being diſcharged. If two equal phials, however, be equally charged at the ſame time, and one of them be plunged into the fixed air, and the other kept out of it; the latter will always retain the charge longer than the former, which will ſometimes retain it but a very ſhort time; owing, as I ſuppoſe, to the *moiſture*, which is readily abſorbed by the fixed air.

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I ALSO found *inflammable* air to be the ſame as common, or fixed air, with reſpect to the power of conducting electricity.

THESE experiments on fixed air, imperfect as they were, led, however, to a diſcovery, which may poſſibly throw ſome new light upon ſome of the moſt fundamental principles of electricity.

BEING at that time but little acquainted with the nature of air, imagining, that fixed air only was unfit for reſpiration, and knowing that air was moſt injured by burning *charcoal*, I thought of trying charcoal itſelf in ſubſtance. Accordingly, on May the 4th, 1766, I tried charcoal, in a variety of ways and ſtates; and found it to be, what I had ſuſpected, an excellent conductor of electricity.

PRESENTING a piece of charcoal to the prime conductor, together with my finger, or a piece of braſs wire, I conſtantly obſerved, that the electric ſpark ſtruck the charcoal before either of the other conductors, if it happened to be advanced ever ſo little before them. Having a very rough ſurface, the charcoal did not take a denſe ſpark from the conductor, till it was made a little ſmooth, and brought within about half an inch; when, to all appearance, it did quite as well as any piece of metal, there being a conſtant ſtream of denſe and white electric fire between the conductor and it. I tried the charcoal in every ſtate of heat or cold, and found no alteration of its conducting power.

I PLACED a great number of pieces of charcoal, not leſs than twelve or twenty, of various ſizes, in a circuit, and diſcharged a common jar through them; when, to all appearance, the diſcharge was as perfect, as if ſo many pieces of metal had been placed in the ſame manner. Two of the pieces, about the middle of the circuit, I placed about an inch and a half from one another; but, upon the diſcharge, the ſpark paſſed the interval very full and ſtrong. A piece of charcoal alſo made the diſcharge at the wire with one ſpark, but the report was not ſo loud as when the diſcharge was made with a piece of metal. It was obſervable, that a black groſs ſmoke roſe from between each of the pieces of charcoal, at the moment of the diſcharge; but the ignition was momentary, and the fire could not be perceived on the charcoal.

To make the experiment of the conducting power of charcoal in the moſt indiſputable manner, I took a piece of baked wood, which I had often uſed for the purpoſe of inſulation, being an excellent non-conductor, and putting it into a long glaſs tube, I thruſt it into the fire, and converted it into charcoal. In this operation, a very great quantity of groſs ſmoke roſe from it, ſo that, ſeeming to part with more of its moiſture, one would have expected it would have come out a better non-conductor; but, upon trial, its electric property was quite gone, and it was become a very good conductor.

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THE experiments above mentioned were firſt made with *wood charcoal*, of which I found pieces of very different degrees of conducting power; but the moſt perfect conductors I have found of this kind are ſome pieces of *pit charcoal*. Theſe ſeem to be, in all reſpects, as perfect conductors as metals. They receive a ſtrong bright ſpark from the prime conductor, though ſeldom at above an inch diſtance, on account of the roughneſs on their ſurface, which cannot be taken off; and in diſcharging a jar through them, or with them, no perſon can imagine any difference between them and metal, either in the colour of the electric ſpark, or the ſound made by the exploſion. When they are broken, they exhibit an appearance which very much reſembles that of broken ſteel. There is however a great variety in the electrical properties of different pieces of this kind of charcoal; and for want of proper opportunity I have not yet ſucceeded in aſcertaining, with ſufficient certainty, the circumſtances, in the preparation, &c. on which this variety depends.

I WOULD have preferred the examination of wood charcoal on many accounts; particularly, as the ſame ſubſtance is, in this caſe, converted from a perfect electric to a perfect conductor; and all the degrees of conducting power may be found in different ſpecimens of it; whereas pit coal is itſelf a conductor, though an imperfect one: but not having any opportunity, I procured ſpecimens of all the varieties I could imagine in the ſame heap of pit charcoal, with reſpect to their nearneſs or diſtance from the ſurface, &c. but though I examined them with all the care and attention that I could apply, and in every method that I could think of, the differences were ſo exceedingly ſmall, if any, that I could not fix upon any circumſtance that I could depend upon for the cauſe of them.

Even common cinders from an open fire, of the kind of coals which we generally burn, I find to be very little inferior to charcoal; which is ſuffered to flame, but covered very cloſe as ſoon as it is well burnt, and before any aſhes are formed. Coals and cinders from a common fire, being a very commodious ſubject for experiments, I did not fail to make as many upon them as I could imagine would be of any uſe; except that I had no opportunity of trying a ſufficient variety of coals. I took ſeveral out of the fire after they had done blazing, ſome of which I covered with aſhes, ſome I quenched in water, and ſome I left to cool in the open air. I alſo reduced ſome of the coals to cinders in a glaſs veſſel, without ſuffering them to flame; and I treated in the very ſame manner various pieces of oak, cut from the ſame plank; but when I examined them, I found their differences, with reſpect to their power of conducting electricity, very inconſiderable, if any. I thought the cinder of a coal which we call kennel, and which is remarkable for flaming much while it burns, to be

a beta better conductor than a cinder from a common coal: but the difference might be owing to its more uniform texture, and smoother surface. Charcoal made of coals which yield a strong sulphureous smell when they are burnt, and of which the charcoal itself is not quite divested, was, to all appearance, as good a conductor as that of the other kind, which is more esteemed.

In this course of experiments I found myself much at a loss for a sufficiently accurate method of ascertaining the difference of conducting substances, and I wish that electricians would endeavour to find such a measure. One of the best that I am acquainted with, and which I applied among others on this occasion, is by the residuum of discharges, measured by Mr. Lane's electrometer. It is well known, that the worse the conductors are that form the circuit, the greater the residuum will be left in a jar after a discharge; and Mr. Lane's electrometer, which measures an explosion, will likewise measure the residuum. To apply this method with accuracy, I put pieces of charcoal, &c. of the same length into the circuit, I used the very same chain in every experiment, and the same disposition of every part of the apparatus; I also made the explosions exactly equal, and after every discharge completed the circuit by the chain before I took the residuum; and lastly, I was careful to take up the same time in each operation, which I repeated very often. This method of measuring the conducting power of ſubſtances I learned of Mr. Lane.

In the proſecution of theſe experiments on charcoal, I burned a piece which I had found to be a moſt excellent conductor, firſt between two crucibles, and then in the open fire, and tried it at different times till it was almoſt burned away; but, contrary to my expectations, I found its property very little diminiſhed. I was, likewiſe, ſurpriſed to find that *ſoot*, whether of wood coal, or pit coal, hardly conducted at all. I made five or ſix inches of the ſoot of pit coal part of the electric circuit, which completed the communication between the inſide and outſide of a charged jar for ſeveral ſeconds; and yet found the charge not much diminiſhed. A piece of wood ſoot, which is a firm ſhining ſubſtance, which does not ſoil the fingers, and which ſeems to break in a poliſh in ſeveral places, would hardly conduct any part of a charge in the leaſt ſenſible degree. When rubbed againſt my hand, or my waiſtcoat in froſty weather (though it was difficult to find any part of it that was large and ſmooth enough for the purpoſe) I more than once thought it attracted the thread of trial. The ſnuff of a candle would not conduct a ſhock, though it was placed in the middle of the circuit, and it was eaſily ſet on fire by the exploſion of a ſmall jar.

But notwithſtanding my want of ſucceſs, I make no doubt, but that any perſon of tolerable ſagacity, who has an opportunity of making

ing experiments in a laboratory, where he could reduce to a coal all kinds ſubſtances, in every variety of method, might very ſoon aſcertain what it is that makes charcoal a conductor of electricity. In all the methods in which I could make charcoal, the fume of the bodies was ſuffered to eſcape; but let trials be made of ſubſtances, reduced to a coal without any communication with the open air, or where the vapours emitted from them ſhall meet with different degrees of reſiſtance to their eſcape, aſcertained by actual preſſure.

CHARCOAL, beſides its property of conducting electricity, is, on many other accounts, a very remarkable ſubſtance; being indeſtructible by any method, beſides burning in the open air; and yet it ſeems not to have been ſufficiently ſtudied by any chymiſt. A proper examination of it promiſes very fair, not only to aſcertain the cauſe of its conducting, and, perhaps, of all conducting powers; but to be an opening to various other important diſcoveries in chymiſtry and Natural Philoſophy; and the ſubject ſeems to be fairly within our reach.

PIT COAL, and probably all other ſubſtances, at the ſame time that they loſe much of their weight, increaſe conſiderably in their bulk in the operation of charring. Does it not ſeem to follow from hence, that its conducting power may poſſible be owing to the largeneſs of its pores, agreeable to the hypotheſis of Dr. Franklin, that electric ſubſtances have exceeding ſmall pores, which diſpoſe them to break with a poliſh.

Or, ſince the calces of metals, which are electric bodies, become metals, and conductors, by being fuſed in contact with charcoal; are not metals themſelves conductors of electricity, in conſequence of ſomething they get from the charcoal?

This courſe of experiments, however, evidently overturns one of the earlieſt, and, hitherto, univerſally received maxims in electricity, viz. that *water* and *metals* are conductors, and all other bodies non-conductors: for we have here a ſubſtance, which is clearly neither water, nor a metal, and yet a good conductor.

N. B. I have ſince found that it is the *degree of heat* with which charcoal is made, on which the *degree of its conducting power* depends. An account of this and other obſervations on charcoal may be ſeen in a paper of mine on the ſubject *.

* Phil. Tranſ. Vol. lx. p. 211.

SECTION

SECTION IV.

EXPERIMENTS ON THE CONDUCTING POWER OF VARIOUS SUBSTANCES.

FINDING ſome contrariety of opinion among electricians about the nature of ICE, ſome ſaying it was a conductor of electricity, and others a non-conductor, ſo as even to be capable of being charged like glaſs, I took the opportunity of a pretty ſevere froſt, in the month of February, to aſſure myſelf of the fact.

IN order to this, I took a large piece of ice, waſhed it very clean, and ſcraped off all the ſharp points about it. After this, when it was again perfectly frozen, I inſulated it, at night, in the open air, whither I had carried my machine on purpoſe, at the ſame time that it was freezing intenſely.

WHEN, by drawing a feather over its ſurface, I found it be perfectly dry, I electrified it, and fetched large ſparks, not leſs than an inch in length, from all parts of it. I charged a jar at it, almoſt as well as at the prime conductor; I alſo diſcharged the jar through it, and along the ſurface of it, in ſeveral places; ſo that I had no doubt, but that ice was, nearly, as good a conductor of electricity as water. To try the ſame to more advantage, I took

took a charged jar into the open fields; and, by means of a great length of chain, diſcharged it along a large ſurface of ice on a pond, whilſt the ſurface was very dry, and the froſt continued very intenſe. But the ice being not ſo good a conductor as metal, if the chain communicating with the outſide of the jar happened to lie five or ſix inches from the knob of the wire communicating with the inſide, the fire would ſtrike to the chain, along the ſurface of the ice, without entering it.

Snow is evidently not ſo good a conductor as ice; probably becauſe its parts do not lie in contact with one another, as thoſe of ice.

Finding alſo that electricians were not perfectly agreed about the conducting power of *hot glaſs*, and that the methods which had been uſed to prove it were liable to objection; ſince, when the electricity was communicated along the outſide of the glaſs, it might be ſaid that the *hot air*, and not the hot glaſs was the conductor; it occurred to me, that the following experiment would determine this affair, in a more ſatisfactory manner than it had hitherto been done.

I procured a glaſs tube, about four feet long; and, by means of mercury in the inſide, and tinfoil on the outſide, I charged about nine inches of the lower part of it. Then carefully ſlipping off the tinfoil, and pouring out the mercury, I heated the charged ed part of the glaſs red-hot; and found, upon replacing the coating, that it was diſcharged.

I MADE the experiment a ſecond time, with the ſame ſucceſs; ſo that I had no doubt, but that glaſs, when red-hot, was pervious to the electric fluid. It could not have gone round from the inſide to the outſide, without going over a ſurface of ſix feet of glaſs, the greateſt part of which was kept very cold, and all of it exceedingly dry.

THAT the charge had not been loſt by changing the quickſilver was evident: for when I repeated that part of the experiment, without heating the glaſs, the charge was found to be very little diminiſhed.

SOME time after, when I was preparing ſome baked wood for the purpoſes of inſulation, I found, that if I uſed them ſoon after they were taken out of the oven, they would not anſwer my purpoſe at all. The electricity went off by them to the floor. But when they had ſtood, in the very ſame ſituation, till they were cold, they inſulated very well.

UPON this, I made a piece of baked wood, which I had formerly uſed for inſulation, pretty hot; and when it was ſo hot, that I could hardly hold it in my hand, it took a ſlender ſpark from the conductor, about an inch long; but it would not diſcharge a jar at once. It did it however ſilently, pretty much like moiſt wood.

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THE confideration of the conducting power of charcoal, and the manner in which it is made, namely, by burning inflammable fubftances, in a clofe place, and generally without flaming, led me to make a few experiments on the conducting power of the effluvia of flaming bodies, at the very time of their emiffion: for whatever thofe effluvia be, they feemed in fome meafure to contain the conducting principle.

THE conducting power of the flame of a candle was obferved very early; but it was not compared with that of other things, and it had by fome been fuppofed to be nothing more than the heat communicated to the neighbouring air. The experiments I am going to recite feem to overturn this hypothefis.

MARCH the 14th, a fmall charged phial held not longer than a fecond within two or three inches of the flame of a candle, either above or below it, where the heat was altogether inconfiderable, and the rarefaction of the air in a manner nothing, was totally difcharged. The event was the fame when I ufed the flame of a wax candle, or the flame of fpirit of wine. When it was held much nearer to a red-hot poker, it was not difcharged near fo foon; and when it was held exceedingly near to a piece of red-hot glafs, it was not difcharged at all, except by one explofion, feemingly conducted by the hot glafs. Similar experiments were made by placing

placing the candle, the poker, and the hot glaſs near the prime conductor. It was alſo found, that the ſmall phial above mentioned could not be diſcharged in the focus of a concave mirror.

But the ſmall jar above mentioned was diſcharged in theſe experiments ſilently; and though they ſeemed to be clearly in favour of the conducting power of the effluvia, which paſs off in flame, there was nothing very ſtriking in them; but afterwards, when I had conſtructed an electrical battery, I repeated the experiments in a much more ſtriking and convincing manner.

December the 15th, I brought the flame of a candle between two braſs knobs, one communicating with the inſide, and the other with the outſide of the battery; and obſerved, that as the flame advanced towards them, it began to be put into a quivering motion, exceedingly quick, and was ſtrongly drawn both ways towards each knob, leaving the wick bare at the top; and as ſoon as the flame was quite between the rods, the battery diſcharged at once, at the diſtance of three inches and an half. This is a very fine experiment. The interpoſition of the flame between the two braſs rods is like putting fire to a train of gunpowder, which explodes immediately.

When I advanced the ignited wick of a candle, juſt blown out, towards the rods, it was ventilated very briſkly; and when it was put between them, when ſeparated to about the

the diſtance of an inch, the diſcharge was made, and the candle blown in again.

To compare the conducting power of flame with that of other bodies, which had more heat but leſs effluvia, I put a red-hot poker between the two rods, but it did not promote the diſcharge of the battery till they were brought within about an inch and an half of one another; ſo that the exploſion was made at about twice the uſual diſtance, allowing for the ſpace occupied by the poker itſelf; and yet the air in the neighbourhood of the poker was more than ten times hotter than in the neighbourhood of the candle conſidering the diſtance at which they were held from the rods. Both ſides of the hot poker were marked with an imperfect circle, like thoſe that were impreſſed on each of the knobs; an account of which will be given hereafter.

I THEN interpoſed a piece of red-hot glaſs, which has as great a heat as the iron, but emits leſs effluvia; but it did not promote the diſcharge till the braſs rods were brought within an inch of one another, which was ſo near, that the glaſs almoſt touched them both.

As I was diverſifying the experiments concerning the paſſage of the electric exploſion over the ſurfaces of various bodies, as will be mentioned hereafter, I accidentally diſcovered how exceedingly poor a conductor is *oil* of every kind; inſomuch that I think it ought rather to be claſſed among electric ſubſtances; though

though before that time, I imagined that oil did not differ very much from water, with respect to it conducting power. I had been led into the mistake by some experiments of Mr. Wilson, who has somewhere advanced the proposition above mentioned; and argues that the tourmalin is possessed of a fixed kind of electricity, incapable of being conducted away, because it retains the separate power of each of its sides, though surrounded with melted grease; whereas I find, that nothing of an oily nature will conduct electricity.

LAYING a chain, which communicated with the outside of my battery, in a dish of melted tallow, I brought a brass rod communicating with the inside towards it, in order to make the discharge, by transmitting the explosion over the surface without entering it; when I was surprised to find, not only that the electric matter would not take the surface, but that, though it attracted a column of tallow at the distance of about three quarters of an inch (which was thicker in proportion as the rod was brought near the surface) and though I continued amusing myself with this column of tallow a considerable time; in which state it formed a complete communication between both sides of the battery, yet the charge was very little dissipated. I repeated this experiment, with the same event, with oil of olives, the thinnest oil of turpentine, and even ether. A plate of common oil of olives connected the inside and outside of the battery for near ten minutes, without my being able to to perceive that the charge was more diſſipated than it would have been without that communication. Ether is the ſlighteſt fluid in nature next to air; yet, being properly an oil, it proved no better a conductor than the moſt tenacious. I was moſt ſurpriſed that the ether did not take fire by this treatment, as nothing is more inflammable; and if the electric matter can paſs through it, nothing fires ſo ſoon.

FROM theſe experiments, and thoſe above mentioned, on ice, I concluded, that fluidity, as ſuch, contributes nothing to the conducting power of ſubſtances, ſeparate from the heat which makes them fluid. To complete my experiments on oils, I filled phials with all kinds of oils, according to their chymical diſtinctions, including the fineſt *eſſential oils*, the ſtrongly *empyreumatic*, and thoſe that are termed mineral, as *oil of amber*; and found them all incapable of giving a ſhock. But I found that this method of trying the conducting power of ſubſtances, viz. by incloſing them in phials, and endeavouring to give ſhocks by them is very inaccurate, ſhowing them to be better conductors than they really are. Pounded glaſs, flower of brimſtone, and other electric ſubſtances gave a conſiderable ſhock; but a bottle containing nothing but air gave a greater ſhock than any of them; though the wire inſerted into it was very blunt, and was kept in the center of the bottle. Finding, by theſe experiments, that oil plainly conducted much leſs than air, I endeavour-

endeavoured to charge a plate of oil like a plate of glafs; and for this purpofe I perforated a glafs falver, and thereby gave a coating of tinfoil to both fides of a quantity of oil poured into it; but the brim of the falver would not contain enough to give it a fufficient thicknefs; otherwife, I make no doubt, but that a fhock might be given by it better than by air.

I SHALL juft mention upon this fubject, what I lately obferved, and do not know whether it has been noticed by any writer, that *ice of oil*, contrary to ice of water, is fpecifically heavier than the fluid fubftance, and finks in it *.

FINDING fo great an agreement, with refpect to electric properties, in this whole chymical clafs of bodies, I began a kind of courfe of chymical electricity; but had not leifure, or opportunity to purfue it as it deferved. The few hints that I collected may poffibly be of fervice to future inquirers; and for this

* OTHER perfons, I find, have made experiments, which fhow how imperfect a conductor oil is. The following proofs of this are exceedingly curious and pleafing. Mr. Cigna obferved electrical attraction and repulfion between conducting fubftances plunged in oil. Nollet's Letters, Vol. iii. p. 168.

MONSIEUR VILLETTE, optician at Liege, filled a difh of metal with oil, and when he had electrified the difh, he plunged a needle into the oil, and received a very ftrong fpark as foon as the point of it came within a fmall diftance of the difh. A fmall cork ball being made to fwim in this oil, upon the approach of the thicker end of the ftalk of a lime, plunged to the bottom, and immediately rofe again to the top. Nollet's Letters, Vol. iii. p. 312.

reaſon I ſhall note them juſt as they occurred, though they contain little that is remarkable.

ALL *ſaline ſubſtances* that I examined proved, in general, pretty good conductors. I tried moſt of them by making the diſcharge of the battery through them when inſulated; which appears to me to be a very good method, indeed the only one that can well be depended upon. In diſcharging the battery with a piece of *alum*, the exploſion was attended with a peculiar hiſſing noiſe, like that of a ſquib. *Rock ſalt* conducted pretty well, but not quite ſo well as the alum. The electric ſpark upon it was peculiarly red. *Sal ammoniac* exceeded them both in its conducting powers, but it would not take the leaſt ſenſible ſpark; ſo that it ſeemed made up of an infinite number of the fineſt points. *Volatile ſal ammoniac* I only tried in a phial, when it gave a ſmall ſhock. *Salt petre* did not conduct ſo well as ſal ammoniac. Endeavouring to make the electric exploſion paſs over its ſurface, it was diſperſed into a great number of fragments in all directions with conſiderable violence. *Selenitic ſalt* conducted a ſhock but poorly. *Vitriolated tartar* gave a ſmall ſhock. *White Sugar* ſeems to be an exception to this rule: for it may be fairly ſaid to be no conductor; as the charge of the battery would hardly paſs through it in the leaſt degree.

THE *metallic ſalts* in general conducted better than other neutrals: *blue and green vitriol*

triol conducted very well, though they would not tranſmit a ſhock.

THAT *ores* in which the metal is really in a metalline ſtate ſhould be very good conductors might naturally be expected. Thus a piece of gold ore from Mexico was hardly to be diſtinguiſhed, in this reſpect, from the metal itſelf; and a piece of ſilver ore from Potoſi, though mixed with pyrites conducted very well. But even ores in which the metal is mineralized with ſulphur and arſenic, as the ores of lead and tin, and *cinnabar* the ore of quickſilver were little, if at all, inferior to them. The cinnabar that I tried was factitious; but there can be no doubt of its being the ſame as the native. When I made the exploſion of the battery paſs through it, it was rent into many pieces, and the fragments diſperſed in all directions. Ores, however, that contain nothing but the earth of the metal conduct electricity but little better than other ſtones; though I thought that all the ſpecimens of iron ore that I tried conducted better than marble *.

I EXAMINED ſome *black ſand* that came from the coaſt of Africa, which is a good iron, and part of which is affected by the magnet as much as ſteel filings; and found it to conduct electricity, but not a ſhock. Separating with a magnet all that would be eaſily

* I FIND that Mr. Boze, very early, thought it was eaſy to diſtinguiſh the ores of metals from other earthy ſubſtances, by means of their greater conducting power. Dantzick Memoirs, Vol. i. p. 293.

attracted by it (about one ſixth of the whole(it conducted a ſhock very well. The reſt would hardly conduct at all.

THOUGH I think I may venture to ſay, that the true and proper ores of the more valuable metals might be known by their property of conducting electricity, I cannot ſay that electricity will furniſh any rule to aſcertain the value of the different ores of the ſame metal. I tried two pieces of copper ore, one the moſt valuable that is known, and another of only about half the value; but they were hardly to be diſtinguiſhed from one another in their conducting power.

BLACK *lead* in a pencil conducted a ſhock ſeemingly like metal or charcoal. A ſmall lump of it took as full and ſtrong a ſpark from the prime conductor as a braſs knob.

ALL the *ſtony ſubſtances* that I tried conducted very well, though dry and warm. Even a piece of poliſhed *agate*, though ſemi-pellucid, received the electric ſpark into its ſubſtance; though it would paſs over about three quarters of an inch of its ſurface to reach the finger that held it, and it diſcharged the battery but ſlowly. *Limeſtone*, and *lime* juſt burnt were equally imperfect conductors, hardly to be diſtinguiſhed from one another. *Lapis hæmatites*, and *touchſtone* both conducted pretty well; as did a piece of *gypſum*, and *plaiſter of Paris*, only the latter, having a ſmoother ſurface, took a ſtronger ſpark. A piece of *ſlate*, ſuch as is commonly uſed to write on, was a much better con-

ductor

ductor than a piece of *free ſtone*, which conducted very poorly. *Marbles* alſo conducted conſiderably better than free ſtone. I found very little difference among any of the ſpecimens of marble that I tried, in which was a piece of Egyptian granite. A piece of Spaniſh chalk, which is a *talc*, conducted pretty much like marble.

A LARGE piece of white *ſpar*, with a tinge of blue, and ſemi-tranſparent, would hardly conduct in the leaſt degree. I took pretty ſtrong ſparks from the prime conductor while it was in contact with it.

A PIECE of *pyrites* of a black colour took ſparks at a conſiderable diſtance from the prime conductor, like ſome of the inferior pieces of charcoal. Another piece of pyrites, which had been part of a regular ſphere, conſiſting of a ſhining metallic matter, did not conduct near ſo well, though much better than any other ſtony ſubſtance. It was a kind of medium between a ſtone and an ore.

A PIECE of *aſbeſtos* from Scotland, juſt as it is taken from its bed, would not conduct. It was in contact with the conductor, while I took ſparks at the diſtance of half an inch with a moderate electrification.

OF *liquid ſubſtances*, *oil of vitriol* conducted pretty well, and the moſt highly rectified *ſpirit of wine* gave a ſhock much like water, but perhaps not quite ſo well.

THIS courſe of experiments on the conducting power of ſubſtances, according to

 their

their chymical claſſes, would, probably, be very uſeful, if purſued with care. Thoſe mentioned above were generally ſingle experiments, which are not ſo much to be depended upon.

THERE are ſome other *mixed ſubſtances* whoſe conducting power I have tried, and becauſe I think it would not be eaſy to ſay, *a priori*, to which of the two claſſes they belong, I ſhall juſt mention the reſult of my experiments upon them, nearly in the order in which they were made.

DRY *glue*, which is an animal ſubſtance, is a conductor of electricity, but does not conduct a ſhock.

POUNDED *glaſs* mixed with the white of an egg, and which had ſtood till it was perfectly dry, was a conductor. I had put it upon ſome broken jars, thinking that the compoſition would be an electric ſubſtance, and that it would make the jars hold a charge again.

PAINT, made of white lead and oil, very old, and dry, proved a conductor. I tried it in a china veſſel which had been firmly pieced with it. A part of the veſſel, through which there was no crack, would receive a charge very well; but a piece in which there was a crack, and which had been filled with this cement, could not be charged at all.

SECTION

SECTION V.

EXPERIMENTS ON THE DIFFUSION OF ELECTRICITY OVER THE SURFACES OF GLASS TUBES, CONTAINING A NEW METHOD OF GIVING THE ELECTRIC SHOCK.

IT had been obſerved by many electricians, that new globes are often difficult to excite; but I have made ſome experiments, which prove this fact, and other differences between new and old glaſs, in a more diſtinct manner than any thing elſe I have yet met with; but they leave the cauſe ſtill unexplained.

THE moſt remarkable property of new flint glaſs is the eaſy diffuſion of electricity over its ſurface. I have ſeveral times got tubes made two or three yards long, terminating in ſolid rods. Theſe I have taken almoſt warm from the furnace, in the fineſt weather poſſible, and having immediately inſulated them, and hanging pith balls at one extremity, have always found, that they would ſeparate the moment that the wire of a charged phial was applied to the other end. This I had reaſon to think would be the caſe at almoſt any diſtance at which the experiment could be made. I have even charged a phial very ſenſibly, when it was held cloſe to the glaſs, at the diftance of a yard from the wire of a charged phial, held clofe to another part of it, the coatings of both phials being held in my hands. When the fame tubes were a few months older, I found that the electric virtue could not be diffufed along their furfaces farther than about half a yard.

SOME tubes, which I have tried the day they were made, I have found impoffible to be excited in the leaft degree, even with the ufe of oiled filk and amalgam, for an hour together; when a fingle ftroke of the fame rubber has rendered other tubes highly electrical, and two or three have made them to emit fpontaneous pencils. The fame new tubes, upon being much rubbed, have begun to be excited, and in a few days have acted pretty well.

BUT that the firft coat of new glafs is, in fome meafure, a conductor of electricity, was moft evident from fome experiments which I made with long and very thin tubes, which were blown fome time in the month of March. Thefe, to amufe myfelf, I coated in different places, and the diffufion of electricity, from the coated part to that which was not coated, appeared to me very extraordinary. I think my reader will not be difpleafed if I relate a few of the particulars.

I PROCURED a tube, open at both ends, about a yard in length, but of very unequal width. About three inches of the middle part of it I coated on both fides; and charging it, by means of a wire introduced at one of the

ends;

ends; I perceived, not only that the part through which I had introduced the wire was ſtrongly electrical on the outſide, but that at the oppoſite end, where there was neither coating nor wire, the fire crackled under my fingers, as I drew the tube through them, and a flame ſeemed to iſſue continually out at both the ends, while it was at reſt and charged. N. B. One end of this tube was broken, and rough, the other was ſmooth.

I PROCURED another tube, about an inch in diameter, and very thin. It was about three feet and a half in length, and cloſed at one end. About nine inches below the mouth, I coated three inches of it, both on the inſide and outſide. This part I charged, and then obſerved the whole tube, to the very extremity of it, to be ſtrongly electrical, crackling very loud when I drew my hand along it, and giving ſparks, as from an excited tube, at about the diſtance of an inch, all the way.

To give the reader a better idea of theſe experiments, I have given a drawing [Pl. I. fig. 7] of one of the tubes with which they were made. It is open at one end, and the part [*a*] is coated.

AFTER drawing the whole tube through my hand, all the electricity on the outſide was diſcharged; but, upon putting my finger within the mouth of the tube, an effort to diſcharge itſelf ſeemed to be produced, which ſhowed itſelf by a light ſtreaming viſibly from the coating, both towards the finger and

and likewife as vigorously towards the oppofite end of the tube. After this I found all the outfide of the tube loaded with electricity as before, which might be taken off, and revived again many times, with the fame original charge; only it was weaker every time.

HOLDING this tube by the coated part, and prefenting the uncoated outfide, near the clofe end of the tube, to the prime conductor, the infide became charged as well as the outfide; and, upon introducing a wire, a confiderable explofion was made.

THE difcharge made the outfide ftrongly electrical, and by taking this electricity off, the tube was charged again very fenfibly.

HOLDING it by the uncoated part, and prefenting the coated part to the conductor, the infide became charged as before.

HAVING firft perfectly difcharged this tube, I clofed the open end with cement, made of bees wax and turpentine, an inch or more in thicknefs; but ftill, by applying the outfide of the tube (either the coated or the uncoated part) to the conductor, I found it manifeftly charged, but not quite fo high as when the end was left open, though the difference was not great.

I PROVIDED myfelf with another tube, about an inch and a quarter wide, and three feet long; but it was drawn out one foot more very fmall; and another foot at the extremity was folid, fo that it was in all five feet long, I coated about four inches of this

tube,

tube, two feet below the mouth of it. The balls being hung at the extremity of this tube, or rather of the ſolid rod in which it terminated, they ſeparated the moment I began to charge the coated part. The diſcharge brought them together, though not immediately, but a ſecond diſcharge would generally do it.

THE reſiduums of any of theſe tubes, of which ſo ſmall a part was coated, were very conſiderable. I thought that all of them might be equal to the firſt diſcharge. In the laſt mentioned tube, there was a reſiduum after a great number of diſcharges, I believe twenty or thirty.

IMAGINING that the diffuſion on the ſurfaces of the tubes above mentioned depended upon the *newneſs* of the glaſs, I preſerved them ſix or ſeven months; having obſerved by examining them at proper intervals in the mean time, that this property, and others depending upon it, gradually leſſened; and before this time it was quite gone. There was no diffuſion of electricity over their ſurfaces, and they were as eaſily excited as other tubes, at the ſame time that they received a very good charge.

AT length, by ſome accident or other, all the tubes on which I had made theſe experiments were broken, except one, which was cloſed at one end, and which indeed, was the moſt remarkable of them all. Upon this tube, in the month of November, I began to renew my experiments, comparing it with

others

tohers which I got made at that time, in order to aſcertain on what circumſtances this diffuſion of electricity depended. Theſe I ſhall diſtinctly relate, noting the time when each experiment was made, and every other circumſtance which I can imagine could poſſibly have any influence in the caſe.

NOVEMBER the 13th, I once more endeavoured to repeat the experiments above mentioned with the old thin tube, with as much care and precaution as poſſible, but without the leaſt ſucceſs. At the ſame time I charged two other thin tubes, one cloſed, and the other open, after they had been made about ſix weeks, but without being uſed in the mean time, and they anſwered exactly as the former tube had done, when it was new. The charge from a ſmall coated part diffuſed itſelf all over the tube; ſo that at the diſtance of a yard from the coating, it gave ſparks to the finger of an inch in length, and in all reſpects exhibited the appearance of a tube freſh excited. On this occaſion I firſt obſerved, what afterwards drew my attention in a more particular manner, that when my finger was brought to the tube about two inches above the coating [as at *b*. Pl. I. fig. 7.] it diſcharged a great quantity of that diffuſed electricity; and my whole arm was violently ſhocked.

NOVEMBER the 19th, After heating the old tube, and endeavouring to repeat the former experiments, both while very warm, and after it was cold again, but to as little purpoſe as before; I took it to the glaſs houſe,

and

and got it made red-hot all over, ſo that it would eaſily bend any way; and as ſoon as it was cold, I tried the old experiments, and found that it had completely recovered its former property. Charging a ſmall coated part, the electricity was diffuſed to the end of the tube, over three feet of dry glaſs, and it gave ſparks at the diſtance of an inch in any part of it, exactly as if it had been excited with the beſt rubber. When it was drawn through my hand, whereby that diffuſion was taken off, it preſently returned again; and the extremity of the tube would get loaded while its communication with the coating had been cut off, by my hand being conſtantly held on the middle of it.

I ALSO obſerved, that the middle part of this tube, which had been ofteneſt heated, in melting the whole over again, one half at a time, had a much ſtronger diffuſion than the other parts. It was no ſooner taken off, than it appeared again, ſo that it gave a continual ſtream of fire.

THE quantity of *reſiduum* after a diſcharge of this tube was prodigious, ſo that the outſide coating would, immediately after, give almoſt a conſtant ſtream of fire to any conductor preſented to it, for a conſiderable time.

THIS tube was now, as it had been at the firſt, abſolutely incapable of being excited with the beſt rubber.

JANUARY the 6th, 1767. Examining all the tubes with which I had made the experiments

ments of the diffuſion, I found that property either quite, or very nearly gone. One of them I reſtored by heating it red-hot. Another I heated only at the end moſt remote from the coating; but there was no diffuſion upon it, when the coated part was charged; the part which had not been made red-hot intercepting it.

NOVEMBER the 24th. In order to determine whether this property of diffuſion depended in any meaſure upon the ſmoothneſs of the ſurface, I made a circular part of one of the thin tubes, about half a yard beyond the coated part, quite rough with emery, about three inches in length; but this did not prevent the diffuſion in the leaſt; both that rough part, and the ſmooth glaſs beyond it were as much loaded with the electricity as the reſt.

I THEN took the poliſh off a line the whole length of the tube, from the coating to the extremity of it; but ſtill the effect was the ſame: and I make no doubt would have been ſo if I had made all the ſurface rough.

IN order to aſcertain whether this property depended upon the thinneſs of the tubes, I got one made of a twelfth of an inch thick, and uſed it immediately; the diffuſion was very ſenſible, and it was incapable of being excited. This, however, was not always the caſe with tubes of ſo great a thickneſs.

NOVEMBER the 25th. Willing to carry this experiment a little farther, I got another tube four feet long, and of the eighth of an inch

inch thick. I coated a ſmall part of it in the ſame manner as I had done the others, namely about three inches, at the diſtance of nine inches below the orifice, and obſerved the diffuſion to be very remarkable in proportion to the charge it received, which was very moderate. It could not be ſenſibly excited in the leaſt degree; except that, in the dark, an exceedingly ſmall light was viſible near the finger, when it touched any part of it, immediately after excitation; but not the leaſt ſnapping could be perceived, nor any thing felt with the finger.

To find whether this property depended upon the *kind*, as well as the newneſs of the glaſs, I afterwards, coated a part of a very thin glaſs of the common bottle metal, but I found no diffuſion upon it at all. It was what is commonly called a ſinging glaſs. I would have purſued this experiment by trying the ſame glaſs in other forms, and by trying other kinds of glaſs, but I had no opportunity.

I OBSERVED, in all the tubes which had the diffuſion, that in drawing my hand from the extremity of them towards the coating, after they were charged, ſo as to take off the diffuſion, there was a conſiderable noiſe at the orifice, as if the tube had been gradually diſcharging itſelf, and this operation did apparently leſſen the charge.

IN the dark the electric fire ſeemed to pour perpetually from the open end, or both the ends, if they were both open; and whenever

ever I drew my hand over it, the fire ſtreamed from the coating towards my hand in a very beautiful manner.

It was very remarkable, that, the firſt time I charged any of theſe tubes after they had ſtood a while, the diffuſion was the moſt conſiderable, and that it leſſened every ſucceſſive charge; till, at laſt, it was exceedingly ſmall; but after the tube had ſtood a few hours uncharged, it was as vigorous as ever.

December the 1ſt. I, for the firſt time, took particular notice in charging a thin tube, and afterwards holding the coating in one hand, and drawing my other hand, ſo as to graſp the tube; beginning at that end which was moſt remote from the coating; that ſometimes, when my hand came near the coating [as at *b*. Pl. I. fig. 7.] I received a very conſiderable ſhock through both my arms and in my breaſt, exactly like what is felt from the Leyden phial.

The ſame day, I felt a ſimilar ſhock from another thin tube; and what was more remarkable, I did not receive it till the third time of drawing my hand over the tube, having miſſed the ſtroke the two firſt times; though I moved my hand, as near as I could judge, in the ſame manner. This ſhock was not very great, but ſenſible in both arms.

December the 3d. I received another ſhock, the third time of drawing my hand over the tube, and much more violent than the

the laſt; it affecting both my arms and breaſt. At this time I obſerved very exactly, that my hand was near two inches and an half from the coating, and that a ſtrong light was viſible under my hand, and extended to the coating. The diffuſion at this time had not been very great, and the tube ſeemed to be about half diſcharged after the ſhock.

At that time I could not think of any plauſible theory to account for this ſhock; but preſently after I accidentally received another ſhock, in ſome reſpects ſimilar to this, the theory of which I have been ſo happy as to inveſtigate, and which may throw ſome light upon this.

December the 21ſt. I made a Torricellian vacuum in a tube about a yard in length [Pl. I. fig. 8.] and holding one end of it in my hand, I preſented a part near the other end to the prime conductor; and obſerved, that, while the electric fire was pouring along the whole length of it, I felt ſome peculiarly ſmart twitches every now and then in my hand, juſt ſuch as are felt when a thin uncoated phial is held in the hand, while it is charged at the prime conductor, but more pungent. On removing the tube from the prime conductor, it threw out ſpontaneous ſparks from the place where it had touched the conductor, exactly like thoſe which iſſue from the wire of an over-charged phial; but they were longer, and much more beautiful. Then, bringing my other hand near the place where the tube had touched the conductor, I received a very conſiderable ſhock in both my arms and breaſt, exactly like that which I had received before from the thin tubes; and, as with them, the ſhock was rather ſtronger in the hand which was brought to the tube than in that which held it. If, without bringing my other hand to the tube, I only preſented it to the table, or any other conductor, it would throw out from the ſame place ſeveral ſtrong ſparks, attended with a flaſh of light, which filled the whole length of the tube. Theſe ſparks reſembled thoſe which iſſue from the wire of a charged phial, when it is preſented to the like imperfect conductors, and at the ſame time held in the hand.

I AFTERWARDS obſerved, that the ſtrongeſt ſhock which this tube could give was felt when one hand continued in the place where it held the tube, in order to charge it, and the other was made to touch the tube, an inch or two above it [as at *c*. Pl. I. fig. 8.] and at the inſtant of the ſtroke a very denſe ſpark of electric fire was ſeen darting the whole length of the tube. When three perſons beſides myſelf joined hands, it ſhook all our arms greatly.

THE tube could not be diſcharged by putting one hand ſo near the other, unleſs that part of the tube had been brought to the conductor in charging it; and if any particular part of the tube only, had been brought to the conductor, the diſcharge could not be made without touching that part.

When the tube had given a ſhock from any one place, it would give one or two more ſmaller ſhocks from other places.

The experiments I made with this tube being certain and invariable, and the ſhock I received from the other tubes precarious, I gave more particular attention to this, in order to aſcertain the nature of this ſhock; thinking that, if I could accompliſh this, it would aſſiſt me in the inveſtigation of the other. Accordingly, I coated about ſix inches, near each end of the tube, [*a* and *b* Pl. I. fig. 8.] leaving the ſpace of about half a yard of uncoated glaſs between the coatings; and obſerved, that when I held one of theſe coatings in my hand, and preſented the other to the prime conductor, it always received a conſiderable charge, and was diſcharged in one bright ſpark at the diſtance of above an inch, and ſometimes two inches, if, beſides the coated part, I had likewiſe preſented the uncoated part to the prime conductor; and ſometimes the uncoated part would diſcharge itſelf by a bright flaſh to the lower coating, leaving the coated part charged as before. If I held the tube by the middle, where there was no coating, and preſented one of the coatings to the conductor, it received a pretty good charge.

I then ſtood upon an inſulated ſtool, and preſenting one of the coatings to be charged, while I held the other; I obſerved, that it received not more than one fourth part of the charge it had before; upon which I immediately concluded, that the lower coating muſt have been charged negatively, whilſt the upper was charged poſitively. This was quite confirmed by obſerving, that ſparks could be drawn from my body, while I ſtood upon the ſtool preſenting the tube to be charged, but no longer than till the tube had received its full charge; and that then the exploſion was as great as it had been when I ſtood upon the floor.

When I inſulated the tube, by placing it in a glaſs veſſel, it was ſtill leſs capable of taking a charge than when I ſtood upon a ſtool and held it, this method making a more perfect inſulation. If any conductor was preſented to the lower coating while the other was held to the prime conductor, ſparks iſſued from it very plentifully till it had got a conſiderable charge; when thoſe ſparks intirely ceaſed, and the tube, upon trial, gave a very great exploſion.

These experiments make the theory of this new method of giving the electric ſhock pretty obvious. The electric matter thrown upon the upper coating repels an equal quantity from the inſide of the tube oppoſite to it; which, paſſing freely through the vacuum (as is viſible in the dark), is accumulated on the inſide of the other extremity of the tube, and thereby repels a quantity from the lower coating: ſo that the two coatings being in oppoſite ſtates, though on the ſame

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ſide of the tube, the ſame kind of ſhock is given by them, as if they had been on oppoſite ſides.

Being fully ſatisfied with the experiments made by theſe two coatings, and the theory of them, I amuſed myſelf with coating the middle part of the tube in various ways.

When the three coatings were about the ſame ſize, and placed at equal intervals; which ever of them was held in the hand, the other two were charged and diſcharged ſeparately. If the coating of one of the ends was held in the hand, and the other two were charged, the greateſt exploſion was from that which was diſcharged firſt. If thoſe two coatings were placed near one another, they were both diſcharged by the attempt to diſcharge either of them, and a flaſh of light was ſeen betwixt them both. In this caſe the exploſion was ſometimes made at the diſtance of two inches and a half.

When the middle coating was made very large, and placed contiguous to the upper, the exploſion was leſs; a ſpontaneous diſcharge being ſoon made to the lower coating.

When the middle coating was taken away, it often happened that, in drawing the whole tube over the prime conductor, beginning at the upper coating; when it came to the lower, by which I held it, a ſpark would dart to it from all the uncoated part of the tube, which diſcharged the electricity of that part, while the upper coating ſtill retained its proper charge.

When this ſpontaneous diſcharge was not made, the exploſion might be made at twice, once at the naked glaſs, near the lower coating, and again at the upper coating. If the diſcharge was firſt made at the upper coating, there remained very little for the lower part of the tube. And if the exploſition was made about the middle of the tube, the whole was diſcharged at once, and in a very beautiful manner.

I must leave my reader to compare the theory of this ſhock with that given by the long and open tubes, as I am not able to do it to my intire ſatisfaction without more experiments; which, as I obſerved, are precarious, and which I had not leiſure to attend to.

As this courſe of experiments was begun by an accidental obſervation of the different electric properties of new and old glaſs, I ſhall (after this long excurſion, which I little foreſaw) conclude with an experiment or two relating more immediately to the original ſubject of them.

Imagining that the difference between new and old glaſs might be owing to the larger ſuperficial pores of the former, which made it approach to the nature of a conductor, and which contracted with time; I thought it might poſſibly determined by the experiment of the metallic tinge, the wider pores receiving it better than the ſmaller;

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and I was not diſappointed in my expectations. November the 19th, I ſeveral times laid two glaſs tubes, one a very old one, and the other quite new, cloſe together, with a piece of leaf gold or copper between them; and though I varied the diſpoſition of them in every way that I could think of, and changed the tubes for others; I always found the new glaſs to receive a much fairer, more beautiful, and indelible impreſſion than the old glaſs. Twice the quantity of the metal was in all the caſes ſtruck into it.

SECTION VI.

EXPERIMENTS TO VERIFY SEVERAL PARTICULARS OF SIGNIOR BECCARIA'S THEORY OF ELECTRICITY; PARTICULARLLY CONCERNING THE ELECTRIC MATTER CARRYING INTO ITS PATH LIGHT SUBTANCES TO ASSIST ITS PASSAGE.

BEING greatly ſtruck with Signior Beccaria's theory concerning the paſſage of the electric matter from the earth to the clouds, previous to a thunder ſtorm, and thinking his experiments to prove the power of electricity to conduct into its path light ſubſtances that could aſſiſt its paſſage not quite ſatisfactory, I endeavoured to aſcertain the fact in a better manner, and ſhall lay before my readers the reſult of my experiments.

NOVEMBER the 9th. I diſcharged frequent ſhocks, both of a common jar, and another of three ſquare feet, through trains of braſs duſt, laid on a ſtool of baked wood, making interruptions in various parts of the train; and always found the braſs duſt ſcattered in the intervals, ſo as to connect the two disjoined ends of the train; but then it was likewiſe ſcattered nearly as much from almoſt all other parts of the train, and in all directions.

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The ſcattering from the train itſelf was probably occaſioned by ſmall electric ſparks between the particles of the duſt; which, cauſing a vacuum in the air, drove all that light matter to a conſiderable diſtance. But the particles of the duſt which were ſtrowed in the intervals of the train, ſome of which were, at leaſt, three inches, could hardly be conveyed in that manner.

When ſmall trains were laid, the diſperſion was the moſt conſiderable, and a light was very viſible in the dark, illuminating the whole circuit. It made no difference, in any of theſe experiments, which way the ſhock was diſcharged.

When I laid a conſiderable quantity of the duſt at the ends of two pieces of chain, through which the ſhock paſſed, at the diſtance of about three inches from one another, the duſt was always diſperſed over the whole interval, but chiefly laterally; ſo that the greateſt quantity of it lay in arches, extending both ways, and leaving very little of it in the middle of the path. It is probable, that the electric power would have ſpread it equably, but that the vacuum made in the air, by the paſſage of the fluid from one heap of duſt to the other, diſperſed it from the middle part.

I then inſulated a jar of three ſquare feet, and upon an adjoining glaſs ſtand laid a heap of braſs duſt; and at the diſtance of ſeven or eight inches a braſs rod communicating with the outſide of the jar. Upon bringing another

other rod, communicating with the inſide, upon the heap of duſt, it was diſperſed in a beautiful manner, but not one way more than another. However, it preſently reached the rod communicating with the outſide.

Making two heaps, about eight inches aſunder, I brought one rod communicating with the inſide upon one of them, and another rod communicating with the outſide upon the other. Both the heaps were diſperſed in all directions, and ſoon met; preſently after which the jar was diſcharged, by means of this diſperſed duſt, in one full exploſion. When the two heaps were too far aſunder to promote a full diſcharge at once, a gradual diſcharge was made through the ſcattered particles of the duſt.

When one heap of duſt was laid in the center of the ſtand, and the two rods were made to approach on each ſide of it, they each attracted the duſt from the ſide of the heap next to them, and repelled it again in all directions. When they came very near the heap, the diſcharge was made through it, without giving it any particular motion.

All theſe experiments ſhow that light bodies, poſſeſſed of a conſiderable ſhare of electricity, diſperſe in all directions, carrying the electric matter to places not abounding with it; and that they ſometimes promote a ſudden diſcharge of great quantities of that matter from places where it was lodged, to places where there was a defect of it. But

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an accident led me to a much more beautiful, and perhaps a more ſatisfactory manner of demonſtrating the laſt part of this propoſition, than any that I hit upon while I was purſuing my experiments with that deſign.

December the 11th. Hanging a drop of water upon the knob of a braſs rod communicating with the inſide of my battery, in order to obſerve what variety it might occaſion in the circular ſpots, which will be mentioned hereafter; I was greatly ſurpriſed to find the exploſion made all at once, at the diſtance of two inches.

I, afterwards, put ſome braſs duſt upon a plate of metal communicating with the inſide of the battery, and making the diſcharge through the duſt, it exploded at the diſtance of an inch and a half. The duſt roſe towards the diſcharge rod, and from thence was diſperſed in all directions.

These experiments are the more remarkable, as they demonſtrate ſo great a difference between the diſtance at which the battery may be made to diſcharge at once, by the help of theſe light bodies, and without them. When the diſcharge of a battery by the knobs of braſs rods, in the open air, is at the diſtance of about half an inch; it will, by this means, be made at about two inches.

Unless a perſon try the following experiment, he will hardly conceive the extreme probability of the clouds and the rain being poſſeſſed of an electric virtue, in order to their

their uniform diſperſion, according to Signior Beccaria's theory. Put a quantity of braſs duſt into a coated jar, and when it is charged, invert it, and throw ſome of the duſt out. It is very pleaſing to ſee with what exact uniformity it will be ſpread over any flat ſurface, and fall juſt like rain or ſnow. In no other method can it be ſpread ſo equably.

It is taken for granted by Signior Beccaria and others, that perſons are ſometimes killed by lightning without being really touched by it, a vacuum of air only being ſuddenly made near them, and the air ruſhing out of their lungs to ſupply it; and with ſo much violence, that they could never recover their breath. As a proof of this, he ſays, that the lungs of ſuch perſons are found flaccid; whereas, when they are properly killed by the electric ſhock, the lungs are found inflated. This account always appeared to me highly improbable. It determined me, however, to make a few experiments, in order, if poſſible, to aſcertain the fact with ſome degree of exactneſs. The reſult was as follows.

December the 18th. I placed that part of an egg ſhell in which is a bladder of air within an inch of the place where I made the exploſion of the battery, on the ſurface of ſome quickſilver; when the bladder was inſtantly burſt, and the greateſt part of it torn quite away. The ſhell was quite dry; ſo ſo that the bladder could not ſtretch in the leaſt.

It is evident from this experiment, that there is a ſenſible expanſion of the neighbouring air, to fill a vacuum made by the electric exploſion; but that this is ſo conſiderable as to occaſion the ſuffocation and death of any animal, is, I think, very improbable from the following facts.

I put a cork as ſlightly as poſſible in the mouth of a ſmall phial; but, though I held it exceedingly near the place of the exploſion, it was not drawn out.

I made the exploſion paſs over the ſurface of a moiſt bladder, ſtretched on the mouth of a galley-pot; but it produced no ſenſible effect upon it. I alſo held at one time the bill of a robin red-breaſt, and at another time the noſe of a mouſe near the electric exploſion, but they did not ſeem to be at all affected by it. In order to examine the ſtate of the lungs, I killed ſmall animals by ſhocks diſcharged both through the brain, and through the lungs; but when they were diſſected there appeared no difference. The lungs were in the very ſame ſtate as when they were killed in another manner.

To theſe miſcellaneous experiments, intended to verify ſeveral particulars of Signior Beccaria's theory of electricity, I ſhall add a ſmall ſet, which, though they were begun begun before I had ſeen tha author, are in ſome reſpects ſimilar to his curious experiment

ment of difcharging a plate of glafs hanging by a filken ftring without giving motion to it; his being defigned to afcertain the effect of the difcharge upon the glafs, and mine refpecting the conducting fubftances that formed the circuit.

OCTOBER the 7th. To determine, if poffible, the direction of the electric fluid in an electric explofion, I hung feveral brafs balls by filken ftrings, and difcharged fhocks through them, when they were as much at reft as I could make them; but I could not perceive that any motion was given to them by the ftroke. Afterwards, I difcharged a jar a great number of times through fmall globules of quickfilver, laid on a fmooth piece of glafs; but could not perceive that they were driven one way more than another, though they were often thrown into diforder; probably by the repulfion of the air, occafioned by the vacuum of the explofion.

I THEN placed four cork or pith balls, at equal diftances, upon a ftool of baked wood, with a piece of chain at the fame diftance from the outermoft balls; and obferved, that, upon every attempt to make a difcharge, the two middle balls were driven clofe together, while the two outermoft were each of them attracted to the piece of chain that was next to it. Then, laying a great number of bits of threads in the fame manner upon the ftool, feveral of the pieces that lay near the chain

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ſtuck to them, and a great number of thoſe that lay in the middle were driven together in a heap.

The attraction to the chains I attribute to the electricity given to them by their connection with the jar, which would be greatly encreaſed by the attempt to make an exploſion; and the crowding together of the pieces in the middle of the circuit, I attribute to the current of air blowing them together from both the extremities of the chain. Thus part of the flame of a candle next to an electrified point will be attracted by the power of electricity, while the reſt of the flame will be repelled from it by the current of air.

These experiments led me to make a diſcharge through an inſulated bell, in order to obſerve in what manner it would be affected by the electric ſhock only, when it was not touched by any thing elſe. Accordingly, I made the diſcharge of the battery through it ſeveral times; and by each exploſion it was made to ring, as loud as it could be made to do with a pretty ſmart ſtroke of one's finger nail.

I also made a diſcharge of the battery through the external coating of a glaſs jar, but without touching it with the diſcharging rods; and it plainly produced the ſame tone, as when it was rung by percuſſion.

SECTION VII.

VARIOUS EXPERIMENTS RELATING TO CHARGING AND DISCHARGING GLASS JARS AND BATTERIES.

AS ſeveral things have occurred, in the courſe of my experiments, relating to charging and diſcharging both common jars and large electrical batteries, which I have not ſeen in the writings of any electricians; and as ſome of the facts are not eaſily accounted for, I ſhall mention a few of the more remarkable of them, juſt as they happened.

APRIL the 28th. As I was amuſing myſelf with charging three jars of the ordinary ſize, while they ſtood upon a metal plate on the table, with their wires at different diſtances from the ſame prime conductor, which was fixed on pillars of baked wood; I obſerved, that whenever one of the jars, which ſtood next to the conductor, diſcharged itſelf, the others would diſcharge themſelves too; though they were far from having received their full charge, being placed at a greater diſtance from the common conductor, and conſequently having taken but few ſparks, in compariſon of that which ſtood the neareſt.

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A VARIETY of experiments ſeem to ſhow, that, while a jar continues charged, the electric matter is continually inſinuating itſelf farther and farther into the ſubſtance of the glaſs, ſo that the hazard of its burſting is the greateſt ſome time after the charging is over.

MAY the 26th. After having charged forty-one jars togethers, each containing about a ſquare foot of coated glaſs, I let them ſtand about a minute and a half, while I was adjuſting ſome part of the apparatus, in order to make the diſcharge; when they exploded by the burſting of one of the number. I obſerved alſo, that the jar which was burſt was at a conſiderable diſtance from the place where I ſaw the flaſh at the wires. It was alſo broke through in two different places.

FOR the ſame reaſon, there is no being ſure that jars, which have ſtood one diſcharge, will bear another equally high. I am confident that ſeveral of mine have burſt with a much leſs charge than they had actually held before.

JUNE the 29th. A jar of an ordinary ſize, which had been in conſtant uſe for ſeveral months, and which had diſcharged itſelf more than a hundred times without any injury, at length burſt, as I was diſcharging it at the prime conductor. The hole was at a different place from that at which the diſcharge was made, but this does not always happen. The tip of my little finger happened to lie very lightly on the place, and I felt it was burſt by a ſmall pricking, as of a pin, though the exploſion at the conductor was nearly equal to that of any other diſcharge. The coating of a jar contiguous to one that is burſt is always melted by the exploſion.

June the 25th. A ſmall thin phial, which had been charged ſingly as high as it could bear, ſo as to have diſcharged itſelf, and alſo in conjunction with four others of its own ſize, burſt by a ſpontaneous exploſion, when it was charged in conjunction with a battery.

I had never heard of a jar burſting in more than one place, or more than one jar in a battery burſting at the ſame time; but I have often found, to my coſt, that this event is very poſſible. In this caſe, there muſt be a diſcharge at more places than one at the ſame time: and, beſides, it ſeems to follow from it, that whenever there is a ſolicitation, as we may ſay, to diſcharge at one place, the effort to diſcharge at every other place is encreaſed at the ſame time.

It has frequently happened with me, that jars have been burſt at the inſtant that I was making the diſcharge in the common way; and when I have come to charge them again, they have appeared to be burſt, in ſome place of the battery where I never expected it. Two inſtances of this kind happened in the exploſion mentioned above; but the moſt remarkable fact of this kind happened the 31ſt of May, when a battery of about forty jars, each

each containing a ſquare foot of coated glaſs, diſcharged itſelf.

Upon examination, I found that ſix of the jars were burſt, one had the tinfoil coating on the outſide quite melted, in a circular ſpot about half an inch in diameter; and in the inſide it was burned quite black, near an inch and an half. A ſecond was melted on the outſide, about three quarters of an inch in diameter, and the black ſpot in the inſide was two inches. A third had one hole made in the form of a ſtar, more ſmall cracks like *radii* proceeded from a center than could be counted. And there was hardly one of the jars that was burſt with a ſingle hole. Some were burſt in ſeven or eight different places, of which ſome were very remote from others; but generally there was one principal hole, and ſeveral ſmaller, but independent ones, in the neighbourhood of it, as within half an inch, an inch, or two inches from it.

June the 14th. The above mentioned battery diſcharged itſelf once more; when three jars were burſt, and one of them, beſides its principal hole, had a circular row of fractures, quite round the hole, at the diſtance of about half an inch. This appearance ſtruck me as ſomething very remarkable, but ſome light may perhaps be thrown upon it by a ſubſequent courſe of experiments. Each of the ſmaller fractures was about a tenth of an inch in length.

November the 17th. Having charged both my batteries, one of them, at that time, of thirty-one ſquare feet, and the other of thirty-two, I made the exploſion; when one jar in each battery was afterwards found to be broken. They broke at the inſtant of the diſcharge, ſo that I did not ſuſpect what had happened. Both the batteries had frequently borne a much higher charge. In one of the ſmaller jars, the coating, beſides being burſt oppoſite to the hole, was rent about an inch and an half along a crack that was made from it.

When jars diſpoſed in batteries have been burſt in this manner. I have never failed to obſerve one circumſtance which appears to me truly remarkable. It is, that though, in this caſe, ſeveral paſſages be opened for reſtoring the equilibrium of the electric fluid, yet the whole ſeems to paſs in the circuit that is formed for it externally. At leaſt, the effect of the exploſion is not ſenſibly diminiſhed, upon any ſubſtances that are expoſed to it. This I had a fair opportunity of obſerving when I was tranſmitting the exploſion of the battery through wires of different metals. I found that the utmoſt force of the battery would do little more than melt a piece of ſilver wire on which I was trying it, and yet it was, at one time, totally diſperſed by an exploſion, in making which three jars were broken in different parts of the battery.

The moſt remarkable fracture I remember, was of a jar an eighth of an inch thick, and which was therefore, for a long time, thought to be too thick for uſe. This jar, however, which

which had never held but a moderate charge, burſt ſpontaneouſly; when there was found in it one hole like what is commonly obſerved, from which extended two cracks that met on the oppoſite ſide of the jar, ſo that it came in two parts: but, beſides this, there were two other holes, barely viſible to the naked eye, at the diſtance of ſome inches from the principal hole, and conſiderably diſtant from one another. Yet theſe holes, when examined with a microſcope, were plainly fractures, like others made in the ſame manner, having a white ſpeck in the middle. One of them was above the external coating, but not above the internal.

I HAD frequently been much ſurpriſed at the great diſtance at which ſeveral of my jars would diſcharge themſelves, one of five inches being very common. This induced me to try at what diſtance I could make that ſpontaneous diſcharge.

FEBRUARY the 21ſt. I got a jar, eight inches and a half in depth, and three in diameter. Finding it diſcharged itſelf very eaſily, when coated in the uſual manner, that is, about four inches from the top; I cut the coating away, till I had brought it within two inches and a quarter from the bottom; when it ſtill retained the ſame property; and, at length, it burſt by a diſcharge through a white ſpeck of unvitrified matter, an inch and three quarters above the top of the coating.

I THEN procured a jar made blue with zaffre, ſeven inches and a half high, and two inches and an half in diameter. I coated of it only one inch and a quarter from the bottom, and yet it diſcharged itſelf very readily. I afterwards, by degrees, cut the coating down to little more than half an inch from the bottom, and ſtill the diſcharge continued to be made as before. This property it retained till the month of October following, when it was broke by an accident.

I HAVE another blue jar, of nearly the ſame ſize with the former, which is almoſt full of braſs duſt, but has no coating at all on the outſide. Yet, if I ſet this jar upon the table, in contact with a ſingle piece of braſs chain, going quite round it, and lying upon the table, it will diſcharge itſelf the whole length of the glaſs. N. B. The manner in which the uncoated part of theſe jars becomes charged exhibits an exceedingly beautiful appearance, eſpecially in the dark; the fire flaſhing from the top of the coating, in the form of branches of trees, firſt on one ſide of the glaſs, and then on the other, and growing larger and larger till they go over the top.

I HAVE made ſome experiments, to try how thick a plate of glaſs may be charged, but I have not been able to aſcertain this circumſtance with any degree of exactneſs: I only found, that I was not able to give the leaſt charge to a plate of glaſs half an inch thick,

thick, when it was not warmed. It was the bottom of a large glafs tumbler; but meeting with it only upon a journey at the houfe of an ingenious electrician, I had no opportunity of making many experiments upon it. I imagine that warming it would have made it capable of being charged. Glafs of a quarter of an inch thick will hold a pretty good charge.

Mr. Kinnersley's experiments leaving me no reafon to doubt, but that Florence flafks were capable of receiving a charge like any other thin glafs, which might be made a conductor by heat, I imagined I could foon conftruct a very ftrong and very cheap electrical battery out of them. Accordingly I procured a few, for a fpecimen, but was greatly furprifed to find that the electricity went through them, when quite cold, like water through a fieve, without making any fracture in them: for they continued to hold the fame fmall charge, which was different in different flafks. Mentioning this difappointment to Mr. Canton, he informed me, that he had met with the fame, and that the permeability of this kind of glafs to the electric fluid was owing to fmall unvitrified parts which may be feen in them. I thought it might be of ufe to publifh this fact, as it may prevent other perfons from being difappointed in the fame expectations.

As glafs had generally been charged when it was fmooth, and electrics which had the property of rough glafs, when excited, were exceedingly difficult to charge; I had the curiofity to try what might be done with *rough glaſs* itſelf. Accordingly, I firſt made part of a jar rough, connecting the inſide with the outſide coating; thinking that the roughneſs might poſſibly promote a ſpontaneous diſcharge; but I found it was not made in that place preferably to any other. I afterwards took the poliſh from all that part of the outſide of a jar that was above the coating, but it was charged and diſcharged exactly as before. Laſtly, I made a plate of glaſs rough on both ſides, taking off all its poliſh, and found that it received a charge as well as a ſmooth plate.

The manner in which tubes and plates of glaſs have broken, when I have failed to ſtrike a metallic tinge into them, by the diſcharge of an electrical battery, have ſometimes been attended with circumſtances which I cannot eaſily account for. The following are the facts.

December the 3d. Endeavouring to fix a metallic tinge upon a flat piece of glaſs, it was broken by the exploſion, parallel to the line along which the metal lay, at about an inch diſtance, but not where the tinge itſelf was.

In attempting to give a metallic tinge to a part of a long glaſs tube, it broke, though not in the place where the tinge was made, but on the oppoſite ſide, which was ſhattered all to pieces. The leaf gold had been bound tight to the glaſs, under a piece of paſteboard, which

which covered the gold, but not all the tube. Another tube alſo was broken in large fragments where the metal had been put on, but into ſmall ſplinters on the oppoſite ſide; and for the ſpace of ſix or ſeven inches farther, it was not broken at all on the ſide of the metal, but very much on the other ſide.

At another time, in attempting the ſame thing with another glaſs tube; the end of it, which was near a foot diſtant from the place where the metal was laid, and which was a little cracked in an oblique direction, broke off in a round piece.

As few experiments have been publiſhed about *melting wires*, and procuring globules of metal by electrical diſcharges, and as ſeveral things have occurred in my attempts that way, which perhaps have not occurred to other perſons, I ſhall mention a few of the moſt material circumſtances. They will, at leaſt, ſerve as a direction to thoſe who may be diſpoſed to attempt the ſame thing.

I had frequently attempted to procure thoſe beautiful *globules of metal*, ſome of which I had ſeen with Mr Canton; and for that purpoſe had made the diſcharge through ſmall wires laid in the bottom of china bowls, &c. but always without ſucceſs. At length, I thought of incloſing the wires in ſmall tubes; and this expedient fully anſwered my purpoſe: for, November the 12th, diſcharging a battery of thirty-two ſquare

ſquare feet through an iron wire incloſed in a ſmall glaſs tube, I found innumerable globules of the metal, of very different ſizes. The whole piece melted was about two inches. Breaking the glaſs tube, I found the inſide ſurface uniformly covered with thoſe globules, and a black duſt, both fixed into the glaſs; ſo that they they could not be ſeparated, without tearing away part of the glaſs.

THINKING to avoid this inconvenience, I fixed the ſmall wire in the center of a glaſs tube, of, at leaſt, a quarter of an inch in diameter; but, upon the diſcharge, this tube, though much wider than the former, was uniformly covered with the globules, and the black duſt, which ſtuck very faſt to it, though the metal did not ſeem to have penetrated into the ſubſtance of the glaſs. When the tube was broken, I ſcraped off part of the black lining, and the part next the glaſs looked like a thin plate of the metal.

IMAGINING that the melted metal would not adhere ſo cloſely to a conducting ſubſtance, as it had to the glaſs, I incloſed the wire in a paper tube a quarter of an inch wide. Upon opening it after the diſcharge, it was found uniformly covered with that black duſt, and the ſtain was every where indelible. Sparks of fire had been ſeen three feet from the place of diſcharge, but no part of the metal could be found.

I THEN confined the wire cloſer, wrapping it tight in paper. Upon the diſcharge, a great

great number of ſparks were ſeen, for about a ſecond of time, a quarter of a yard from the paper, which was burned through in ſeveral places. Very few pieces could be found, but thoſe were pretty large and irregular. I now found, that, in order to produce theſe globules, the charge muſt be moderate, that when the charge was very high, the whole ſubſtance of the wire was diſperſed in particles too ſmall to be found; and, on the other hand, when the charge was not ſufficient, the metal was melted into fragments too large to form themſelves into regular globules.

WITH the ſame battery I once melted a piece of iron wire one ſeventieth of an inch in diameter, when a piece of it was thrown quite acroſs the table, to the diſtance of about ſix feet; where it fell upon a bureau, then tumbled down to the ground, and continued glowing hot all the time. At other times, ſparks from melted iron have been thrown three yards, in oppoſite directions, from the place of the fuſion, and continued a ſenſible ſpace of time red-hot upon the floor.

AT another time I had a very fine opportunity of obſerving what part of the conductors which form an electric circuit are moſt affected by the exploſion: for, upon diſcharging a battery of fifty-one ſquare feet through an iron wire nine inches long, the whole of it was glowing hot and continued ſo for ſome ſeconds; the middle part growing cool

cool firſt, while both the extremities were ſenſibly red. Upon examining it afterwards, both the extremities were found quite melted; an inch or two of the part next to them were exceedingly brittle, and crumbled into ſmall pieces upon being handled; while the middle part remained pretty firm, but had quite loſt its poliſh, ſo that it looked darker than before.

SECTION

SECTION VIII.

EXPERIMENTS ON ANIMALS.

AS I have conſtructed an electrical battery of conſiderably greater force than any other that I have yet heard of, and as I have ſometimes expoſed animals to the ſhock of it, and have particularly attended to ſeveral circumſtances, which have been overlooked, or miſapprehended by others; it may not be improper to relate a few of the caſes, in which the facts were, in any reſpect, new, or worth notice.

JUNE the 4th. I killed a rat with the diſcharge of two jars, each containing three ſquare feet of coated glaſs. The animal died immediately, after being univerſally convulſed, at the inſtant of the ſtroke. After ſome time, it was carefully diſſected; but there was no internal injury perceived, particularly no extravaſation, either in the abdomen, thorax, or brain.

JUNE the 19th. I killed a pretty large kitten with the diſcharge of a battery of thirty-three ſquare feet; but no other effect was obſerved, except that a red ſpot was found on the pericranium, where the fire entered. I endeavoured to bring it to life, by diſtending the lungs, blowing with a quill into the trachea,

trachea, but to no purpoſe. The heart beat a ſhort time after the ſtroke, but reſpiration ceaſed immediately.

June the 21ſt. I killed a ſmall field-mouſe with the diſcharge of a battery of thirty-ſix ſquare feet, but no other effect was perceived, except that the hair of the forehead was ſinged, and in part torn off. There was no extravaſation any where, though the animal was ſo ſmall, and the force with which it was killed ſo great. This fact, and many others of a ſimilar nature, make me ſuſpect ſome miſtake, in caſes where larger animals are ſaid to have had all their blood veſſels burſt by a much inferior force.

In all the accounts that I have met with of animals killed by the electric ſhock, the victims were either ſmall quadrupeds, or fowls; and they are all repreſented as killed ſo ſuddenly, that it could not be ſeen how they were affected previous to their expiration. In ſome of my experiments, the great force of my battery has afforded me a pretty fair opportunity of obſerving in what manner the animal ſyſtem is affected by the electric ſhock, the animals which I have expoſed to it being pretty large; ſo that a better judgment may be formed of their ſenſations, and conſequently of the immediate cauſe of their death, by external ſigns. I do not pretend to draw any concluſion myſelf from the following facts. I have only noted them as carefully as I could for the uſe of phyſicians and anatomiſts.

June

June the 26th. I difcharged a battery of thirty-eight fquare feet of coated glafs, through the head, and out at the tail of *a full grown cat*, three or four years old. At that inftant, fhe was violently convulfed all over. After a fhort refpite, there came on fmaller convulfions, in various mufcles, particularly on the fides; which terminated in a violent convulfive refpiration, attended with a rattling in the throat. This continued five minutes, without any motion that could be called breathing, but was fucceeded by an exceedingly quick refpiration, which continued near half an hour. Towards the end of this time, fhe was able to move her head, and fore feet, fo as to pufh herfelf backwards on the floor; but fhe was not able to move her hind feet in the leaft, notwithftanding the fhock had not paffed through them. While fhe continued in this condition, I gave her a fecond ftroke, which was attended, as before, with the violent convulfion, the fhort refpite, and the convulfive refpiration; in which, after continuing about a minute, fhe died.

Being willing to try, for once, the effect of a much greater fhock than that which killed the cat upon a large animal, I gave an explofion of fixty-two fquare feet of coated glafs to a dog of the fize of a common cur. The moment he was ftruck, which was on the head (but, not having a very good light, I could not tell exactly where) all his limbs were extended, he fell backwards, and lay with-

without any motion, or ſign of life for about a minute. Then followed convulſions, but not very violent, in all his limbs; and after that a convulſive reſpiration, attended with a ſmall rattling in the throat. In about four minutes from the time that he was ſtruck, he was able to move, though he did not offer to walk till about half an hour after; in all which time, he kept diſcharging a great quantity of ſaliva; and there was alſo a great flux of rheum from his eyes, on which he kept putting his feet; though in other reſpects he lay perfectly liſtleſs. He never opened his eyes all the evening in which he was ſtruck, and the next morning he appeared to be quite blind, though ſeemingly well in every other reſpect.

HAVING diſpatched the dog, by ſhooting him through the hinder part of his head, I examined one of his eyes (both of which had an uniform blueiſh caſt, like a film over the pupil) and found all the three humours perfectly tranſparent, and, as far as could be judged, in their right ſtate; but the *cornea* was throughout white and opaque, like a bit of griſtle, and remarkably thick.

BEFORE this experiment, I had imagined, that animals ſtruck blind by lightning had probably a *gutta ſerena*, on account of the concuſſion which is ſeemingly given to the nervous ſyſtem by the electric ſhock; whereas this caſe was evidently an inflammation, occaſioned by the exploſion being made

made ſo near the eyes, terminating in a ſpecies of the *albugo*; but which I ſuppoſe would have been incurable. One of the eyes of this dog was affected a little more than the other; owing, probably, to the ſtroke being made a little nearer to one eye than the other. I intended to give the ſtroke about an inch above the eyes.

In order to aſcertain the effects of electricity on an animal body, I, after this, began a courſe of experiments on the conducting power of its conſtituent parts; and for ſome time imagined that a piece of ſpinal marrow of an ox conducted ſenſibly worſe than the muſcular fleſh; but after a great number of trials with pieces of ſpinal marrow from various animals, and pieces of muſcular fleſh, of the ſame ſize and form, and in various ſtates of moiſture and dryneſs, I gave up that opinion as fallacious; but I cannot help wiſhing the experiments were reſumed with ſome more accurate meaſure of conducting power than hath yet been contrived.

Being willing to obſerve, if poſſible, the immediate effect of the electric ſhock on the heart and lungs of animals, I gave, June the 5th, a ſhock from ſix ſquare feet to a frog, in which the thorax had been previouſly laid open, ſo that the pulſation of the heart might be ſeen. Upon receiving the ſtroke, the lungs were inſtantly inflated; and, together with the other contents of the thorax, thrown quite out of the body. The heart, however, continued to beat, though very languidly, and there was no other ſign of life for about ten minutes. After that, a motion was firſt perceived under its jaws; which was propagated, by degrees, to the muſcles of the ſides; and at laſt the creature ſeemed as if it would have come to life, if it had not been ſo much mangled. The ſtroke entered the head, and went out at the hind feet.

June the 6th. I diſcharged a battery of thirty-three ſquare feet through the head and whole extended body of another frog. Immediately upon receiving the ſtroke, there was, as it were, a momentary diſtention of all the muſcles of the body, and it remained ſhrivelled up in a moſt ſurpriſing manner. For about five minutes there appeared no ſign of life, and the pulſation of the heart could not be felt with the finger. But afterwards, there firſt appeared a motion under the jaws, then all along the ſides, attended with convulſive motions of the other parts, and in about an hour it became, to all appearance, as well as ever.

The ſame day, I gave the ſame ſtroke to two other frogs. They were affected in the ſame manner, and perfectly recovered in leſs than three hours.

These facts ſurpriſed me very much. I attribute the recovery of the frogs partly to the moiſture, which always ſeems to cover their

their body, and which might tranſmit a good part of the ſhock; and partly to that proviſion in their conſtitution, whereby they can ſubſiſt a long time without breathing. To aſcertain this, I would have given the ſhock to toads, ſerpents, fiſhes, &c. and various other exanguious animals, but I had not an opportunity. Beſides, it is paying dear for philoſophical diſcoveries, to purchaſe them at the expence of humanity.

SECTION IX.

Experiments on the CIRCULAR SPOTS made on pieces of metal by large electrical explosions.

IN the courfes of experiments with which I fhall prefent the reader in this and the two following fections, I can pretend to no fort of merit. I was unavoidably led to them in the ufe of a very great force of electricity. The firft new appearance was, in all the cafes, perfectly accidental, and engaged me to purfue the train; and the refults are fo far from favouring any particular theory or hypothefis of my own, that I cannot perfectly reconcile many of the various phenomena to any hypothefis.

From the firft of my giving any particular attention to electrical experiments, I entertained a confufed notion, that a perfon would ftand the beft chance for hitting upon fome new difcovery, by applying a greater force than had hitherto been ufed. Confidering the prodigious number of electrical machines that were in the hands of fo many ingenious men, in different parts of the world, I imagined that all that could be done *in little* had been tried; and that the ufual experiments had been diverfified and combined in

almoft

almoſt every method poſſible; whereas, ſince electrical machines, I obſerved, had, of late years, been gradually reduced into leſs and leſs compaſs, a great power of electricity would be almoſt a new thing, and might therefore ſupply the means of new experiments. Even Dr. Franklin's force, I conſidered, was ſmall, in compariſon of what might eaſily be raiſed, and without a very great expence.

WITH theſe general and random expectations, I kept gradually increaſing my quantity of coated glaſs, till I got a battery of thirty, forty, ſixty, and at length near eighty ſquare feet; and the reader will, in ſome meaſure, have ſeen already, that I was not wholly diſappointed in it. The following courſes of experiments are more remarkable inſtances of the advantage I derived from this power of electricity.

THE firſt remarkable fact that I was by this means led to diſcover, is that of the circles with which pieces of metal that receive electrical exploſions are marked. I ſhall faithfully relate all the circumſtances, and varieties in which it has been exhibited, and the obſervations I have made upon it; and this I cannot do better than by writing the narrative, in the order in which the appearances occurred.

JUNE the 13th, 1766. After diſcharging a battery, of about forty ſquare feet, with a ſmooth braſs knob, I accidentally obſerved upon it *a pretty large circular ſpot*, the center of which ſeemed to be ſuperficially melted, in a great number of dots, larger near the center, and ſmaller at a diſtance from it. Beyond this central ſpot was a circle of black duſt, which was eaſily wiped off; but, what I was moſt ſtruck with was, that, after an interruption of melted places, there was an intrie and exact circle of ſhining dots, conſiſting of places ſuperficially melted, like thoſe at the center. The appearance of the whole, excluſive of the black duſt, is repreſented, Plate I. fig. 5. No. 1.

June the 14th. I took the ſpot upon ſmooth pieces of lead and ſilver. It was, in both caſes, like that on the braſs knob, only the central ſpot on the ſilver conſiſted of dots diſpoſed with the utmoſt exactneſs, like *radii* from the center of a circle, each of which terminated a little ſhort of the external circle.

Examining the ſpots with a microſcope, both the ſhining dots that formed the central ſpot, and thoſe which formed the external circle, appeared evidently to conſiſt of *cavities*, reſembling thoſe of the moon, as they appear through a teleſcope, the edges projecting ſhadows into them, when they were held in the ſun.

The moſt beautiful appearance of this kind was exhibited by a ſpot, which I took on a gold watch caſe. Beſides the cavities, there were, in ſeveral places of the ſpot, hollow *bubbles* of the metal, which muſt have been raiſed when it was in a ſtate of fuſion.

Theſe

Theſe looked very beautiful when examined with a microſcope in the ſun, and were eaſily diſtinguiſhed from the cavities, by having their radiant points (which were very remarkable, and dazzling to the eye) on oppoſite ſides to thoſe of the cavities, with reſpect to the ſun. The whole progreſs ſeems to have been firſt a fuſion, then an attraction of the liquid metal, which helped to form the bubbles; and laſtly the burſting of the bubbles, which left the cavities. N. B. By this exploſion half an inch of a ſteel wire, one ſeventieth of an inch in diameter, was melted, and entirely diſperſed. In the diſperſion, ſparks of it were ſeen red-hot, above half a yard from the place where the wire had lain. This circumſtance I have frequently obſerved ſince.

I TOOK the circular ſpot upon poliſhed pieces of ſeveral metals, with the charge of the ſame battery, and obſerved that the cavities in them were ſome of them deeper than others, as I thought, in the following order, beginning with the deepeſt, *tin*, *lead*, *braſs*, *gold*, *ſteel*, *iron*, *copper*, *ſilver*.

I WILL not be very poſitive as to the order of ſome of the metals, but ſilver was evidently not affected a fourth part ſo much as gold, and much leſs than any of the others. The circles were marked as plain, but the impreſſion was more ſuperficial. Qu. Is this owing to the heat being ſooner diffuſed equably through a piece of ſilver, than through the ſubſtance of any other metal?

I THOUGHT there might poſſibly be ſome difference in the circles on metals which had been a long time in a ſolid form, and thoſe which had been lately fluid; and to aſcertain it, I made the exploſion between a piece of lead juſt ſolid after melting, and another ſmooth piece, that I had kept a conſiderable time.

THE piece of freſh lead was melted more than the other, but there was no other difference between them.

THE ſemi-metals, as *biſmuth* and *zink*, received the ſame impreſſion as the proper metals; being melted about as much as iron.

I MADE three diſcharges between a piece of highly poliſhed ſteel, and a piece of very ſmooth iron; and in all the caſes thought the ſteel was more deeply melted than the iron. I mention this experiment more particularly, on account of the ſingular, and beautiful appearance of the circular ſpot upon the ſteel in two of the diſcharges. A circular ſpot, of about an eighth of an inch in diameter, was uniformly melted, and pretty well defined; and there was a ſpace round this central ſpot, of the ſame breadth, uniformly filled with ſmall melted places; but in one of them twice as large as in the other. They exhibited the exact appearance of a planet ſurrounded by a denſe atmoſphere; ſuch as, I think, I remember ſeeing the figures of, in the plates of Burnet's Theory of the Earth. The other circle upon the ſteel was a common one.

WHEN

When the kitten above mentioned was killed, there was no circular ſpot, or any fuſion on the braſs knob. I have always found it the moſt perfect, when the circuit has been compoſed of the beſt conductors, and had the feweſt intervals.

June the 19th. Hanging the caſe of one watch upon the braſs knobs communicating with the inſide of the battery, and receiving the exploſion from it upon the caſe of another watch, which was ſometimes of the ſame, and ſometimes of a different metal, and meaſuring the circles afterwards; I found them to be very nearly of the ſame diameter. The ſmall varieties ſeemed to be accidental; or at leaſt did not depend, either upon the metal, or the direction of the electric fluid. But I thought it pretty evident, from a great number of experiments, that the metal which communicated with the outſide of the battery, and which I held in my hand to take the exploſion, was marked the more diſtinctly of the two.

It ſeemed that, when the battery was charged very high, the central ſpot was the moſt irregular, many of the dots which compoſed it ſpreading into the outer circle, and ſome dots appearing beyond the outer circle, and very much effacing it; ſo that the beſt way to procure a diſtinct circle, is to take a moderate charge of a very large battery. This may be the reaſon why the outer circle cannot be perceived, when only ſmall jars are uſed; the circumference of the circle being

very

very ſmall, and the charge generally too high. In a very weak charge, it is too faint to be perceived. I have ſometimes, however, ſeen a very diſtinct circle made by only two jars, each containing half a ſquare foot of coated glaſs.

The *diameter* of the ſpot ſeems to depend upon the quantity of coated glaſs; but in what proportion, I have not yet accurately aſcertained.

I have obſerved a good deal of variety in the external circles. Sometimes they have conſiſted of pretty large dots, diſpoſed at nearly equal diſtances, in an exact circle; which, in the ſpaces betwixt each large dot, was completed by ſmaller dots, viſible only by a microſcope. But, generally, the external circle conſiſts of a ſpace full of dots, placed irregularly, but ſo that a line drawn through the midſt of them makes a pretty exact circle round the central ſpot.

Presently after I had obſerved the *ſingle circle*, I imagined that, whatever was the cauſe of the appearance, it was not improbable, but that *two or more concentric circles* might be procured, if a greater quantity of coated glaſs was uſed, or perhaps if the exploſion was received upon metals that were more eaſily fuſed than braſs. Accordingly, June the 27th, taking the moderate charge of a battery, conſiſting of about thirty-eight ſquare feet, upon a piece of tin, I firſt obſerved a ſecond outer circle, at the ſame diſtance from the firſt, as the firſt was from the central

central ſpot. It conſiſted of very fine points hardly viſible, except when held in an advantageous light; but the appearance of the whole was very beautiful, ſuch as is repreſented, Plate I. fig. 5. No. 2.

JUNE the 28th. I got another double circle, on a flat pewter ſtandiſh, much plainer than the former, the outer being about the ſame diſtance from the inner, as the inner was from the outſide of the central ſpot.

HAVING hitherto found the circles the moſt diſtinct on metals that melt with the leaſt degree of heat, I ſoon after procured a piece of that compoſition which melts in boiling water; and having charged ſixty ſquare feet of coated glaſs, I received the exploſion with it, and found, what I was endeavouring to get, *three concentric circles*; the outermoſt of which was not quite ſo far from the next to it, as that was from the innermoſt. All the ſpace within the firſt circle was melted; but the ſpace was very well defined, and by no means like a central ſpot, which in this caſe was quite obliterated. The appearance of theſe three concentric circles is repreſented, Plate I. fig. 5. No. 3.

I HAVE ſeveral times ſince found parts of three concentric circles upon braſs knobs, when I have uſed no more than thirty ſquare feet of coated glaſs. They ſeem to be more eaſily perceived, when the knobs are a little tarniſhed: for then the ſmall dots, in which the metal is melted, are more eaſily diſtinguiſhed,

guiſhed, eſpecially when they are held in a proper light with reſpect to the ſun.

I MADE many attempts to make theſe circles larger than I had uſually found them upon pieces of metal, chiefly by means of worſe conductors; thinking that the electric matter not being ſo well conducted, and paſſing with leſs rapidity, would ſpread wider. This was probably the caſe, but then it is likewiſe probable, that I wanted force to make ſuch an impreſſion viſible. For this purpoſe, however, I received the exploſion between two pieces of raw fleſh, two potatoes, two moiſt bladders, and things of a ſimilar nature, but without any effect whatever; no mark at all, or, at leaſt, nothing regular remaining upon them. When I took the exploſion upon a piece of wood charcoal, it ſeemed to be melted, and run in ſmall heaps, within the ſpace of about the uſual diameter of a circular ſpot; and when I took it upon a piece of pit charcoal, a piece ſeemed to be ſtruck out of it, and a hole was left in it; but there was no regular circle upon either of them; nor was there any ſenſible ignition in either caſe.

AT one time I laid a piece of *lead ore*, ſcraped very ſmooth, upon the wires of the battery, and took the exploſion with a piece of tin ore ſcraped in the ſame manner; but though I examined the places with a microſcope, I could not be ſure that there was any part melted, much leſs any regular circular ſpot; but there lay on both of them a yellow matter, like ſulphur, round the place where the explofion was taken, and a very difagreeable fmell was excited. This probably arofe from a mixture of the fulphur of the lead ore, and the arfenic of the tin ore.

I RECEIVED the explofion *in vacuo*, at the diftance of about three inches; but found no regular circular fpot, owing, probably, to the two interruptions I was obliged, in this cafe, to make in the circuit, one in the air, and the other within the receiver; by means of which the effect of both would be weakened, the whole force being, as it were, divided between them: for in all fuch cafes, though both the explofions were made in the open air, I found the circles lefs perfect.

AFTERWARDS, I contrived to make the explofion in one additional atmofphere of condenfed air, but the circles were fmaller, and lefs diftinct than the other two circles, which I was obliged, at the fame time, to make at the other interruption of the circuit, in the open air. The denfer air would probably confine the electric matter within a narrower compafs; in the fame manner as the common air prevents that diffufion of it which is remarkable *in vacuo*.

THE diftance at which the difcharge was made occafioned no difference in the diameter of thefe circular fpots. When, by putting a drop of water upon the brafs rod communicating with the infide of the battery, I made the difcharge at the diftance of two inches, the fpot was juft the fame as if it had been received ed at the diſtance of half an inch, i. e. about a quarter of an inch in diameter.

I ALWAYS found that if the exploſion was obliged to paſs through any bad conductor before it reached the metal, the impreſſion it made upon it was contracted, and deeper than if it had been received immediately by the metal. This was evident when paper, a piece of bladder, or varniſh were put upon the braſs rods with which the diſcharges were made; though a very thin coating of varniſh or moiſture did not intirely prevent the appearance of the circles.

IN making a courſe of experiments with bad conductors, and in uſing various methods to promote the diſcharge of the battery at greater diſtances than uſual, I was peculiarly ſtruck with ſome phenomena which occurred in the uſe of water.

I PUT a drop of water, about a quarter of an inch in diameter, upon the braſs rod communicating with the inſide of the battery, and took the exploſion directly over it. The diſcharge was made at the diſtance of about an inch, and the extremity of the drop was marked with a moſt beautiful circle, exceedingly well defined on the inſide, and vaniſhing gradually outwards, like a fine ſhade in drawing. But what ſtruck me moſt in the appearance was, that, in this circle, there was no central ſpot.

NOT knowing what this new circumſtance was owing to, I wetted a piece of ſmooth copper,

copper, which lay upon the wires of the battery, and taking the explofion upon it, I only found a long ftreak at the edge of the wetted place, well defined on the fide of the water, but vanifhing gradually on the oppofite fide, as in the former cafe. In this, and other fimilar experiments, I obferved that the electric matter avoided the water, and would go a greater way in the air, in order to come at the metal.

I THEN laid more water upon the copper, but fo as only to moiften it; for the furface, being convex, would not allow it to lie in any great quantity; and upon taking the explofion, I found no circle, but feveral beautiful circular fpots melted very deep, one of which was much larger than the reft. Thefe experiments feem to fhow, that the electric matter meets with a confiderable refiftance in paffing through water, which confines its excurfion more than the air; and that, by fuch a condenfation, its force is greatly increafed, fo as to leave deeper impreffions upon the metal than when it had paffed only through the air; in like manner, if two pieces of metal be placed, nearly in contact, or if they be light, and one of them lie upon the other, the impreffion made upon both of them, by the difcharge of the battery paffing through them, will be confiderably deeper than it would have been if the electric matter had not been confined to fo fmall a compafs as the points in contact.

To

To account for the formation of theſe concentric circles, nothing ſeems to be neceſſary, but the ſuppoſition of the elaſticity of the electric fluid, whereby its particles repel one another. For then, ſuppoſing a quantity of electric matter to iſſue from one piece of metal to another, through the air, it will endeavour to ſpread, but will be confined in its paſſage by the ſurrounding electric medium, and the ſtrong attraction of the oppoſite metal. If this piece of metal have a flat ſurface, or one that is nearly ſo, the fluid will be attracted by it pretty equally, within a certain ſpace; ſo that the mutual repulſion of its particles will have room to exert itſelf, and produce a diviſion of the whole quantity; and as this repulſion is the ſame in all directions, the effect muſt be its throwing itſelf into a circle, or ſeveral concentric circles, on its entering the oppoſite piece of metal, and conſequently melting it, in that form. For the ſame reaſon the circles themſelves will conſiſt of ſeparate dots, each of which might have been cauſed by the fluid in another hollow circle, but being ſo ſmall, the fuſion of the metal could not ſhow that circumſtance.

Of the circles being formed in this manner, I have been in a manner an eye witneſs, when I have preſented a flat piece of metal to a large prime conductor ſtrongly electrified, and have ſeen the large ſparks, five or ſix inches long, divide about the middle, and ſtrike the metal in a circle, about an inch in diameter;

ter; generally with a central ſpot, but ſometimes without one.

The manner in which ſeveral of the jars, mentioned in a former ſection, were broken ſeems to be analogous to the formation of theſe circles. I mean thoſe that were pierced with a number of ſmall holes in the neighbourhood of the principal one; but more eſpecially that which was broken with an intire circle of ſmall and independent fractures round the principal hole.

The remarkable ſtory of the five peaſants of whom the firſt, third, and fifth were killed by lightning, as they were walking in a right line; and which was mentioned before, as analogous to a fact obſerved by Mr. Monnier, will perhaps be thought more analogous to this. For ſuppoſing the diameter of the concentric circles formed by lightning to be ſufficiently great, and the central ſpot to fall upon the third perſon, the two on each ſide of him would eſcape, by being in the firſt interval round the central ſpot; while the two who walked firſt and laſt would fall into the circumference of the firſt circle. But other facts lead me to think, that all theſe effects may have been produced by a conſtant ſtream of electric matter, in a progreſſive motion, the firſt part of which being diſcharged upon any object, the ſtream is weakened when it meets with the ſecond, but is accumulated again when the third comes in its way, and ſo alternately, as long as the ſtream and the line of objects continues. This account ſeems to be rendered more probable by the number of objects that have been ſtruck in this manner, particularly in a caſe which is related by Dr. Wallace in his account of the Orkney iſlands, p. 78. " In the year 1680," he ſays, " the lightning entered a gentleman's " cow-ſtall where were twelve cows ſtand- " ing ſide by ſide, as they uſed to be, and " killed every other one; that is, it kill- " ed the firſt, and miſſed the ſecond; it " killed the third and miſſed the fourth; " and ſo of the reſt, ſo that ſix were kill- " ed, and ſix remained alive and untouch- " ed."

Communicating this experiment to Dr. Price, he ſuggeſted to me, that the circles called *fairy rings*, which conſiſt of graſs of deeper green in paſture fields, and which have by ſome been imagined to be occaſioned by lightning, might be analogous to the circles above mentioned, but that they want a central ſpot. I have ſince examined one of theſe rings. It was about a yard in diameter, the ring itſelf about a quarter of a yard broad, and equally ſo in the whole circumference; but there was no appearance of any thing to correſpond to the central ſpot.

I have ſince met with a curious article in the Philoſophical Tranſactions, relating to thoſe fairy circles, communicated by Mr.

 Jeſſop,

Jeſſop, which confirms the ſuppoſition of their being occaſioned by lightning, and with which I ſhall therefore conclude this ſection.

" I HAVE often been puzzled to give an ac-
" count of thoſe phenomena, which are com-
" monly called fairy-circles. I have ſeen
" many of them, and thoſe of two ſorts; one
" ſort bare, of ſeven or eight yards diameter,
" making a round path ſomething more than
" a foot broad, with green graſs in the middle;
" the others like them, but of ſeveral big-
" neſſes, and encompaſſed with a circumfer-
" ence of graſs, about the ſame breadth,
" much freſher and greener than that in the
" middle. But my worthy friend Mr. Wal-
" ker, gave me full ſatisfaction from his own
" experience; it was his chance one day, to
" walk out among ſome mowing graſs (in
" which he had been but a little while before)
" after a great ſtorm of thunder and lightning;
" which ſeemed by the noiſe and flaſhes to
" have been very near him: he preſently ob-
" ſerved a round circle, of about four or five
" yards diameter, the rim whereof was about
" a foot broad, newly burnt bare, as the co-
" lour and brittleneſs of the graſs roots did
" plainly teſtify. He knew not what to
" aſcribe it unto but to the lightning,
" which, beſides the odd capricios re-
" markable in that fire in particular, might
" without any wonder, like all other fires,

" move round, and burn more in the ex-
" tremities than the middle. After the
" grafs was mowed, the next year it
" came up more frefh and green in the
" place burnt, than in the middle, and at
" mowing-time was much taller and ran-
" ker *."

* Phil. Tranf. abridged, Vol. ii. p. 182.

SECTION

SECTION X.

EXPERIMENTS ON THE EFFECTS OF THE ELECTRICAL EXPLOSION DISCHARGED THROUGH A BRASS CHAIN, AND OTHER METALLIC SUBSTANCES.

FROM the very firſt uſe of my battery, I had obſerved a very *black ſmoke* or *duſt* to ariſe upon every diſcharge, even when no wire was melted, and the braſs chain I made uſe of was of a conſiderable thickneſs. Of this circumſtance, however, I only made a ſlight memorandum, as what I could not then account for, and paid no particular attention to it; till on the 13th of June 1766, I was ſtruck with another caſual appearance, as I was intent upon the experiments relating to the circles above mentioned.

I OBSERVED, that a piece of white paper, on which lay the chain I was uſing to make the diſcharge, was marked with a *black ſtain*, as if it had been burnt, wherever the links had touched it. Yet I could not then think that it could be burnt by ſo thick a chain. I imagined the chain muſt have been dirty, and the dirt have been ſhaken off by the ſtroke. Still however I neglected the experiment till, obſerving a very ſtriking appearance of the ſame kind, on the 1ſt of September follow-

 ing,

ing, I was determined to attend to the circumſtances of it a little more particularly than I had done.

I MADE my chain very clean, and wrapping it in white paper, I made a diſcharge of about forty ſquare feet through it, and found the ſtain wherever it had touched the paper.

SOME time after, I wrapped the paper, in the ſame manner, round a piece of braſs wire; but, making a diſcharge through it, ſaw no ſtain. To aſcertain whether this appearance depended upon the diſcontinuity of the metallic circuit, on the 13th of the ſame month, I ſtretched the chain with a conſiderable weight and found the paper, on which it lay as the ſhock paſſed through it, hardly marked at all.

FINDING that it depended upon the diſcontinuity, I laid the chain upon white paper, making each extremity faſt with pins ſtuck through the links; and when I had made the diſcharge, obſerved that the black ſtains were oppoſite to the *body of the wire* that formed the chain, and not to the *intervals*, as I had ſometimes ſuſpected.

SEPTEMBER the 18th. Obſerving that a pretty conſiderable quantity of black matter was left upon the paper, on every diſcharge with the ſame chain; I imagined it muſt have loſt weight by the operation; and to aſcertain this circumſtance, I took another chain not ſo thick as that I had uſed before. It was five feet four inches long, and weighed exactly ly one ounce, ſeventeen penny-weights, four grains. After the diſcharge, I found it had loſt exactly half a grain of its weight. The ſhock had only paſſed through a part of it, the reſt lying on a heap. I then diſcharged the ſame ſhock through its whole length, and weighed it, found it had loſt juſt another half grain. By repeated experiments I found, afterwards, that the ſureſt way to ſtrike off part of its weight, was to make the ſhock paſs through a ſmall part of its length, and that when a conſiderable length was uſed the event was uncertain.

N. B. THESE and all the following experiments, except where the contrary is expreſſed, were made with a battery of *thirty-two ſquare feet*, that force appearing to be ſufficient, and the charging of it not taking up much time. At the time of both the above mentioned diſcharges, an iron wire of one ſeventieth of an inch in diameter was made red-hot, but was not melted.

OBSERVING how deep a ſtain was made by the links of a thick braſs chain, I had the curioſity to try what would be the conſequence of ſending a ſhock through a piece of charcoal. Accordingly I took a ſmall piece, about half an inch in length, and found that, in the diſcharge, it was all blown to duſt. The paſteboard on which it lay was torn, the charcoal being forced into it, ſo that the impreſſion appeared on the other ſide. The blackneſs was ſpread to a great diſtance, and the tinge every where indelible.

September the 21ſt. In making the mark above mentioned, on part of the ſheet of paper, on which I had written an account of the experiment to Dr. Franklin, I happened to lay the chain ſo as to make it return at a ſharp angle, in order to impreſs the form of a letter on the paper; and obſerved that, upon the diſcharge, the part of the chain that had been doubled was diſplaced, and pulled about two inches towards the reſt of the chain. At this I was ſurpriſed, as I thought it lay ſo, as that it could not ſlide by its own weight. Upon this I repeated the experiment with more accuracy. I ſtretched the whole chain along a table, laying it double all the way, and making it return by a very ſharp angle. The conſequence always was, that the chain was ſhortened about two inches, and ſometimes more; as if a ſudden pull had been given to it by both the ends.

Considering that this pull muſt have been given to it by the ſeveral links ſuddenly repelling one another, at the inſtant of the exploſion, I compared the links with the black marks that were made by them upon the table, and found that each link had been pulled from the place on which it had lain, and moſt of all, at the greateſt diſtance from the place of the exploſion.

Convinced that the chain had been ſhortened by the mutual repulſion of the links, I endeavoured to meaſure with exactneſs how much the ſhortening was, in a given length of chain. To do this, I meaſured two feet four

four inches of the chain, as it lay upon the table, in one ſtraight line, without any return, one end being fixed and the other moveable; and found that, upon diſcharging ſixty-four ſquare feet through it, it was ſhortened a quarter of an inch in its whole length. I had contrived that the ſuddenneſs of the motion ſhould not throw one part of the chain upon the other.

Suspecting that the black ſmoke, which roſe at every diſcharge, might come, not from the chain, but from the paper, or the table on which it lay, and which was probably burnt by the contact of it, I let the chain hang freely in the air; but, upon making the diſcharge, I obſerved the ſame black groſs ſmoke that had before riſen from the paper or the table. It was therefore part of the metal itſelf, which had been converted into that black duſt.

To give my reader a better idea of the mark made upon white paper by a chain, through which the electric ſhock is tranſmitted, I laid a chain upon the original drawing of Plate I. for the engraver to copy as exactly as he could; and he has ſucceeded pretty well. The breadth of the ſpots are about the mean thickneſs of the wire of the chain, and [*a*, *b*] marks the place to which that part of the chain which was returned was thrown back, by the ſudden repulſion of the links.

I had before obſerved the electric ſparks betwixt each link to be moſt intenſely bright, ſo ſo as, ſometimes, to make the whole chain appear like one flame in the dark; but the appearance of the chain at the inſtant of the ſhock, as it hung freely in the air, was exceeedingly beautiful; the ſparks being the largeſt and brighteſt at the bottom, and ſmaller, by degrees, towards the top, where they were ſcarcely viſible; the weight of the lower links having brought them ſo much nearer together.

September the 26th. Being ſtill in ſome doubt whether the blackneſs that was left on the paper came from the burning of the paper, or ſomething that was thrown from the chain; I once more hung the chain freely in air, and put under it, but ſo as not to touch it, a piece of white paper, on which I alſo laid a few pieces of down, to obſerve whether they would be affected by any electrical attraction or repulſion. On making the diſcharge, the down was all diſperſed, and the paper was marked with a black ſtain, near the length of an inch; which was the diſtance at which the two parts of the chain hung from one another, a little above the paper. Some parts of the ſtain were deeper than others, the whole mark conſiſting of four different ſpots of a deeper black, joined by fainter ſtreaks, anſwering to four links of the chain, which hung nearly parallel to the paper. The ſtain could not be wiped off with a handkerchief, though it was not ſo deep as when the chain had touched the paper.

paper. Thus I was satisfied, that a considerable part, at least, of the blackness had come from the chain.

SEPTEMBER the 27th. Willing to ascertain more exactly what part of the chain, the solid links, or the intervals, was most affected by the shock; I dipped it in water, and laying it quite wet upon a piece of white paper, discharged a shock through it. Part of the water was thrown into my face, being scattered in all directions, and all the chain left instantly and perfectly dry. The paper was very much stained for the space of an inch broad, wherever the chain lay; not equably, but as if it had been handled with dirty fingers. The stain was indelible, and where the chain was returned, a hole was struck quite through the paper.

To determine whether the paper, in the above mentioned instances, had really been *burnt*, as well as *stained*. I laid a part of the chain, at the time of the last discharge, upon three half crowns; and found they were all melted, in the places where the chain had touched them. The marks made by the fusion were about the breadth of the chain, and so deep that nothing but a tool could efface them.

To determine, if possible, more sensibly what it was that made the black tinge, I laid the chain upon my hand, when I had a moderate charge; and it was marked just like the paper. I felt a kind of pricking or burning at the instant of the explosion, and the painful

painful ſenſation continued a ſmall ſpace of time.

I MADE no doubt but that with a heat that melted metals, I could eaſily contrive to fire gunpowder; but, though I laid the chain upon the grains, and rammed the powder about the chain put through a quill, I could not ſucceed. In the firſt caſe, the powder was diſperſed; and in the ſecond, the quill was burſt, and there was a ſmell, as after an exploſion of gunpowder, but no actual firing of it.

HITHERTO I had always put the chain in contact with bodies that were conductors. I was now willing to try what would be the conſequence of laying it in contact with electrics. Accordingly, I dipped the chain in melted roſin, till it had got a coating of a conſiderable thickneſs. When it was quite ſtiff, I laid it carefully, without bending, upon white paper, and made the diſcharge through it. The roſin was inſtantly diſperſed from all the outſide of the chain, it being left as clean as if none had ever been put on. That with which the holes in the chain had been filled, having been impelled in almoſt all directions, was beaten to powder; which, however, hung together, but was perfectly opaque; whereas it had been quite tranſparent, before this ſtroke. I felt ſome of the roſin fly in my face. The ſtain upon the paper was very deep, containing a good deal of roſin, and ſeveral holes were ſtruck through the paper on which it was laid. A half crown, on which part

part of the chain had lain, was melted, and ſo deeply ſtained with the roſin, that it could not eaſily be cleaned.

I NEXT laid the chain upon a *piece of glaſs*; and conſidering how both the half crown and the roſin had been affected, expected it would have been broken to pieces; but inſtead of that, the glaſs was marked in the moſt beautiful manner, wherever the chain had touched it; every ſpot the width and colour of the link. The metal might be ſcraped off the glaſs at the outſide of the marks; but in the middle part it was forced within the pores of the glaſs; at leaſt nothing I could do would force it off. On the outſide of this metallic tinge was the black duſt, which was eaſily wiped off.

I HAVE ſince given the ſame tinge to glaſs with a ſilver chain, and ſmall pieces of other metals; but could not do it with large pieces. They were melted where they touched one another, but the glaſs was not tinged.

OCTOBER the 7th. I had the curioſity to try, whether I could not give a tinge to glaſs with quickſilver. In order to this, I laid ſome globules in a right line, and laid a thin piece of glaſs upon them, to flatten them, and bring them nearer into contact with one another. Both the ſlips of glaſs were ſhattered in a thouſand pieces, and diſperſed all over the room, ſeveral of them flying in my face; though no part of the quickſilver could be found,

found, except what adhered to ſome fragments of the glaſs, to which it had given a kind of uniform whiteneſs; but no diſtinct globules could be ſeen, and it was eaſily wiped off, ſo that no part of it was fixed in the glaſs. My head ached all the remainder of the day, which I attributed to the fumes of the mercury.

September the 28th. Having dipped the chain in water, and found it inſtantly diſperſed, I wiſhed to ſee what would be the effect of diſcharging a ſhock through a chain quite covered with water. Accordingly, little imagining the conſequence, I laid the chain upon a piece of white paper, in the bottom of a china diſh, and poured in water juſt ſufficient to cover it. Alſo, under one part of the chain, and in the water, I put a half crown. Upon the exploſion, the water was blown about the room, to a great diſtance, the half crown was melted in two places, the diſh broken into many pieces, and the part that lay immediately under the chain into very ſmall fragments. The paper was a little ſtained, and the water, I could perceive, had been a little fouled by the black duſt.

Being certain that the diſh muſt have been broken by the concuſſion given to the water by the electric ſpark under it, in the manner in which Signior Beccaria's tubes were broken (though I had not ſeen his work at that time, but had ſeen the experiment at

Mr.

Mr. Lane's *) I had the curioſity to try what would be the effect of making a diſcharge through the chain hanging freely in water. I therefore got a tin veſſel, holding a quart, and letting the chain hang three inches and a half below the ſurface of the water, made the diſcharge. The electric ſparks appeared intenſely bright in the water, all along the chain; ſome of the water was thrown out, and the veſſel appeared to have been preſſed with ſome force upon a book, which I had put under it, a viſible impreſſion being made upon it. The veſſel muſt have received a great concuſſion: for the duſt had been ſhaken from the bottom upon the book, though I had carried the veſſel up and down the room, without perceiving that any duſt adhered to it.

I was willing to repeat this experiment with ſome variation of circumſtances, and faſtened a piece of ſmall ſilver wire to two pieces of ſtrong braſs wire, and plunged the whole an inch or two under the ſurface of the water. Upon the diſcharge, the ſilver wire was melted, at leaſt ſnapped aſunder, the veſſel had been preſſed downwards more violently than before, a conſiderable quantity of the water was thrown about the table, and ſome was daſhed perpendicularly upwards, againſt the top of the room; where there were

* See his account of it, Phil. Tranſ. Vol. lvii. p. 458.

five wet places, each about the bigneſs of a half crown. I have ſince frequently melted wires under water, and have even made large pieces of iron wire red-hot in the water.

September the 29th. I made a diſcharge through three pieces of the ſame chain, each being a different circuit. They all left their impreſſion upon the paper, and nearly equal. Alſo three out of four pieces made pretty equal marks, but the fourth failed intirely.

At another time, a chain, which communicated with the outſide of the battery, but which made no part of the circuit, made the black ſtain on a piece of white paper on which it accidentally lay, almoſt as deep as the chain that formed the circuit I was then melting a piece of wire, which had the ſame effect as uſing a bad conductor. The ſame thing has frequently happened ſince.

November the 12th. I put a chain through a glaſs tube, ſo wide as that it could only touch one ſide; and upon the diſcharge, obſerved four ſets of marks, made by the metal being driven into the glaſs; as if four chains had been in the tube, and all had received the ſhock. Two of the rows, on one of which I imagine the chain had lain, were better marked than the other two, but all were very plain.

The laſt thing that engaged my attention with reſpect to this courſe of experiments, was

was that *black duſt* which I have obſerved to be diſcharged from the braſs chain, and other pieces of metal. As it was ſo extremely light as to riſe like a cloud in the air, ſo as ſometimes to be viſible near the top of the room; I concluded that it could not be the metal itſelf, but probably the *calx*, or the calx and *phlogiſton*, in another kind of union than that which conſtitutes a metal; and that the electric exploſion reduced metals to their conſtituent principles as effectually as any operation by fire could do it, and in much leſs time. I was confirmed in this opinion by finding, in the firſt place, that this black duſt collected from a braſs chain would not conduct electricity, which is known to be a property of the calces of metals, and alſo by the reſult of ſome of the following experiments.

Considering this black duſt as a proof of calcination, and obſerving it to be produced when I made the exploſions for the circular ſpots between gold and ſilver watch-caſes, as was related above; I began to think I had made a calcination of thoſe metals, which all the chymiſts ſay is impoſſible: but the following experiments convinced me, that it could only be the alloy that was in them which had yielded the black duſt or calx.

Sensible that my experiments with theſe metals would conclude nothing, unleſs I got the ſpecimens quite pure, I firſt procured a

 ſmall

ſmall quantity of *grain gold*, which I was informed was the pureſt that the goldſmiths know, and diſcharged an exploſion of the battery through a train of the pieces, an inch and a half in length, laid on a piece of white paper. Only two of the larger grains could be found after the exploſion. Two leaves of paper were burnt, or torn through in ſeveral places, and more would probably have been torn in the ſame manner, if I had uſed more. But what I principally attended to was the tinge that was given to the paper, with a view to which I had made the experiment. The paper was ſtained near an inch on each ſide of the train, with black intermixed with red, making an odd motley appearance.

With the ſame view, I laid a ſimilar train of bits cut with a knife from a piece of as pure ſilver as I could procure. They were diſperſed, and the paper burnt through, in the ſame manner as with the gold; and the ſpace of about an inch on each ſide of the train was ſtained with black intermixed with a deep yellow, which was conſiderably different from the tinge made by the fuſion of the gold.

The blackneſs in theſe tinges convinced me, that there had been a calcination of ſome part of the metal; but I was convinced it muſt have been ſome alloy, by an experiment I preſently after made with a piece of leaf gold; which, I believe, is generally the pureſt that

that can be got. A ſmall ſlip of this I put through a quill, letting a part hang out at each end; and when I had made the diſcharge through it, I found the quill tinged with a beautiful *vermillion* red, without the leaſt intermixture of black. When I diſperſed a ſlip of leaf braſs in the ſame manner, the greater part of the tinge was black, with a little brown mixed with it in a few places.

In order to aſcertain whether the black duſt was a pure calx, or contained a portion of the metal, I procured a ſmall quantity of it, by ſending an exploſion through ſome pieces of iron wire, ſometimes put into a quill, ſometimes laid upon white paper, and ſometimes upon glaſs, or incloſed in glaſs tubes; but could never be quite ſure that there was any part of it that was not affected with the magnet, which the mere calx would not have been.

Some of the experiments with the braſs chain, related in this ſection, are ſimilar to one of Mr. Wilſon's, mentioned vol. I. p. 121, concerning bodies placed without the electric circuit being affected with the exploſion. As to the cauſe of this, and the other appearances above mentioned, I have no conjecture worth communicating to the public. I have only purſued the analogy of facts, and that not very far. Others may compare them, purſue them farther, and aſcertain their cauſes.

Mr. Canton has ſince clearly proved the calcination of pure gold and ſilver by the heat of electrical exploſions, producing numberleſs moſt beautiful globules of tranſparent glaſs, and alſo others tinged with all the varieties of colour from thoſe metals. He has alſo made it probable, that the *black duſt* mentioned in this ſection, is the calx, or glaſs of the metal, reduced to ſmaller particles than the laws of optics require to produce colour.

SECTION XI.

EXPERIMENTS ON THE PASSAGE OF THE ELECTRICAL EXPLOSION OVER THE SURFACE OF SOME CONDUCTING SUBSTANCES, WITHOUT ENTERING THEM.

I OBSERVED, in relating the experiments on ice, that, in my attempts to aſcertain its conducting power, I ſometimes ſaw the flaſh of the electrical exploſion ſtrike directly to the chain, along the ſurface of the ice. But as this paſſage on the ſurface was produced only by a common jar, it was not much greater than the diſtance at which the diſcharge was uſually made, and the appearance did not ſtrike me. But afterwards the ſame phenomenon occurred in the uſe of my battery, where the paſſage over the ſurface ſo far exceeded the uſual diſtance of a common diſcharge, that it engaged my attention in a very particular manner, and produced ſome pleaſing experiments; which I ſhall recite in the manner, and nearly in the order in which they happened.

DECEMBER the 11th. Thinking to make a circular ſpot on a piece of raw fleſh, I took a leg of mutton, and laying the chain that communicated with the outſide of the battery over the ſhank of it took the exploſion on the outward membrane, about ſeven inches from the chain; but was greatly ſurpriſed to obſerve the electric fire not to enter the fleſh, but to paſs, in a body, along the ſurface of it, to come to the chain.

Thinking that this effect might be occaſioned by the fatty membrane on which the exploſion was taken, I again laid the chain, in the ſame manner, over the ſhank, and took the exploſion upon the fibres of the muſcles, where they had been cut from the reſt of the body; but ſtill the fire avoided entering the fleſh, made a circuit of near an inch round the edge of the joint, and paſſed along the ſurface, to come to the chain as before, though the diſtance was near a eleven inches.

Imagining this effect was promoted by the chain lying lightly on the ſurface of the fleſh, and therefore not really in contact with it; I took another exploſion, when the hook of the chain was thruſt into the fleſh; on which the fire entered the mutton, and, as I held it in my hands, both my arms were violently ſhocked up to my ſhoulders; whereas, in the caſes of the electric fire paſſing over the ſurface of the fleſh, my fingers, happening to touch the chain, were only affected with a ſlight pricking, or ſuperficial burning, which has been explained before.

This phenomenon being ſo remarkable, and the battery by this means diſcharging at at a diſtance about twenty times greater than it could uſually be made to do, I thought to try other ſubſtances, of a conducting power ſimilar to that of raw fleſh; and of theſe, *water* was the moſt obvious. Accordingly, the next day, I laid a braſs rod communicating with the outſide of the battery very near the ſurface of a quantity of water (to reſemble the chain lying upon the ſurface of the fleſh, without being in contact with it) and, by means of another rod furniſhed with knobs, made a diſcharge on the ſurface of the water, at the diſtance of ſeveral inches from any part of the rod; when the electric fire ſtruck down to the water, and, without entering it, paſſed viſibly over its ſurface, till it arrived at that part of the rod which was neareſt to the water, and the exploſion was exceedingly loud. If the diſtance at which I made the diſcharge exceeded ſeven or eight inches, the electric fire entered the water, making a beautiful ſtar upon its ſurface, and yielding a very dull ſound.

THE reſemblance between this paſſage of the electric matter over the ſurface of the water, and that which Dr. Stukeley ſuppoſed to ſweep the ſurface of the earth, when a conſiderable quantity of it is diſcharged to the clouds during an earthquake, immediately ſuggeſted to me, that the water over which it paſſed, and which was viſibly thrown into a tremulous motion, muſt receive a concuſſion, reſembling that which is given to the waters of the ſea on ſuch an occaſion.

To try this, myſelf, and other perſons who were preſent, put our hands into the water, at the time that the electrical flaſh above mentioned paſſed over its ſurface; and we felt a ſudden concuſſion given to them, exactly like that which is ſuppoſed to affect ſhips at ſea during an earthquake. This percuſſion was felt in various parts of the water, but was ſtrongeſt near the place where the exploſion was made.

Afterwards, I made the exploſion of a jar, containing three ſquare feet of coated glaſs, at ſome diſtance below the ſurface of the water, ſo as to be viſible in the water, and we felt the ſame concuſſion that we had done before, when the fire of the battery paſſed over the ſurface, only much weaker. The flaſh of electric fire in the water does certainly diſplace ſome of it, and thereby give a ſudden concuſſion to the reſt; and the ſimilarity of the effect is a conſiderable evidence of a ſimilarity in the cauſe.

I afterwards made the fire of a jar paſs through the water, making a ſpace of about a foot part of the circuit; when, putting our hands in its paſſage, they were affected, but in a very different manner from what they were before: for this evidently affected the nerves and muſcles of the hand internally, and occaſioned a ſmall degree of the ſame kind of convulſion which is felt by the electric ſhock itſelf; whereas the other was a mere percuſſion, affecting the ſurface of the hand. Both ſenſations were, indeed, felt moſt ſenſi-

bly

bly at the ſurface of the water, though our hands were, in ſome meaſure, affected by both as low as we could put them.

Being willing to experience what kind of a ſenſation this paſſage over the ſurface would occaſion, I laid a chain in contact with the outſide of a jar lightly on my finger, and ſometimes kept it at a ſmall diſtance, by means of a thin piece of glaſs; and, if I made the diſcharge at the diſtance of about three inches, the electric fire was viſible on the ſurface of the finger, giving it a ſudden concuſſion, which ſeemed to make it vibrate to the very bone; and when it happened to paſs on that ſide of the finger which was oppoſite to the eye, the whole ſeemed perfectly tranſparent in the dark. If I took the diſtance much larger, the fire entered the finger, occaſioning a very different ſenſation from the former. The one was like a blow, but of a very peculiar kind, whereas the other is well known to be a convulſion.

I then ventured to put my fingers upon a piece of the ſpinal marrow of an ox, while the exploſion of the battery was paſſing over it, when I felt only a ſlight pricking, or percuſſion on each ſide of my finger; and the ſenſation continued for ſome time. This ſenſation did not extend at all beyond the place of percuſſion; but afterwards, putting two of my fingers on the ſame piece of ſpinal marrow, when the charge of the battery was conſiderably ſtronger, I received a concuſſion which

which affected my whole hand, but it was with a kind of a vibratory motion.

Pleased with this reſemblance of the earthquake, I endeavoured to imitate that great natural phenomenon in other reſpects; and it being froſty weather, I took a plate of ice, and placed two ſticks, about three inches high, on their ends, ſo that they would juſt ſtand with eaſe; and upon another part of the ice I placed a bottle, from the cork of which was ſuſpended a braſs ball by a fine thread. Then, making the electric flaſh paſs over the ſurface of the ice, which it did with a very loud report, the nearer pillar fell down, while the more remote ſtood; and the ball, which had hung nearly ſtill, immediately began to make vibrations about an inch in length, and nearly in a right line from the place of the flaſh.

I afterwards diverſified this apparatus, erecting more pillars, and ſuſpending more pendulums, &c. ſometimes upon bladders ſtretched on the mouth of open veſſels; and at other times, on wet boards ſwimming in a veſſel of water. This laſt method ſeemed to anſwer the beſt of any; for the board repreſenting the earth, and the water the ſea, the phenomena of them both during an earthquake may be imitated at the ſame time; pillars, &c. being erected upon the board, and the electric flaſh being made to paſs either over the board, over the water, or over them both. This makes a very fine experiment.

When

When I firſt made this experiment of the electric flaſh paſſing over the ſurface of water, I thought it neceſſary, that neither the piece of metal communicating with the outſide, nor that communicating with the inſide of the jars ſhould touch the water immediately before the diſcharge. But I afterwards found, that the experiment would anſwer, though either, or even both of them were dipped in the water: for in this caſe the exploſion would ſtill prefer the ſurface to the water itſelf, if the diſtance was not very great; and would even paſs at a greater diſtance along the ſurface, when there was a nearer paſſage from one rod to the other in the water.

Just before the diſcharge, both the rods were obſerved to attract the water very ſtrongly. It was thrown upon the rod communicating with the outſide when it was laid near half an inch above the ſurface. When I put a drop of water on the rod communicating with the inſide, the diſcharge was made at the diſtance of about two inches from the ſurface of the water, the fire firſt deſcending perpendicularly, and then paſſing along the ſurface; and if the rod communicating with the outſide had a drop of water upon it, it might be placed higher over the water than if it had not. At the time of the exploſion, this drop was elongated, and promoted the diſcharge very conſiderably.

My attention was next drawn to the kind of impreſſion which was made upon the water by the paſſage of the electric fluid in this manner.

manner. To aſcertain this, I firſt placed a ſhilling level with the water, to receive the exploſion before it paſſed along the ſurface; and obſerved that it was melted, but only about half as much as I imagined it would have been in the common way. There was no regular circular ſpot. And I could never perceive that the braſs rod which communicated with the outſide of the battery was at all melted by the exploſion.

JUDGING from the concuſſion given to the whole body of the water over which the ſhock paſſed, I thought that the trace of it might poſſibly be preſerved on the ſurface of ſoft paſte; and accordingly I made the exploſion paſs over the ſurface of ſome, and plainly obſerved, that the part under the paſſage was depreſſed; the electric matter having repelled it. The impreſſion was not deeper where the exploſion firſt fell than in any other part of the track.

To diſtinguiſh more accurately between the effect of the electric matter when it probably enters the water, and when it only paſſes over the ſurface, I ſpread a little water, exceedingly thin, upon the ſurface of a ſmooth piece of ſlate; but, though the exploſion paſſed over the ſurface, with its uſual violence, I could not perceive that it had occaſioned the leaſt degree of evaporation; which Signior Beccaria found to be the conſequence of making the electrical exploſion through water in ſuch circumſtances.

WHEN

When the explosion passed over the surface of the plate of ice, in the experiment of the earthquake above mentioned, the ice seemed to be melted, both where the chain had been laid, and also along the track over which the explosion had passed. But this melting, if it was such, was not uniform; but looked as if a chain with small links had been laid hot upon it; and the impression was not at all deeper where the explosion was first received.

When the explosion passed over the surface of a green leaf, the leaf was rent in two directions; the longer in the track of the explosion, and the other at right angles to it.

I several times made the explosion on the surface of snow, when it always dispersed a considerable quantity of it, making a hole near two inches deep, and almost as broad as long; for it could not be made to pass at a greater distance than about three inches.

I was not a little surprised to find that I could not make this electrical explosion pass equally over the surface of substances which were conductors in nearly the same degree; and for a long time imagined, that this property was peculiar to water, or to bodies that conducted by means of the water they contained. I could never make it pass the surface of any kind of charcoal; though all the degrees of conducting power may be found in different pieces of it; and I was the more con-

confirmed in my opinion, by obſerving, that, though the exploſion paſſed perfectly well over the ſurface of a ſmooth board, that had been juſt wetted, and immediately wiped as clean as poſſible; yet two hours after, when the board was quite dry, it would not paſs at all in the ſame place. It alſo paſſed with great violence over the ſurface of a bladder which had been moiſtened about a quarter of an hour before, and then ſeemed to be quite dry; but would not paſs in the leaſt degree two or three hours after. In the former caſe, the exploſion had left a mark where it had paſſed over, darker than the reſt of the ſurface, a kind of poliſh which was on it being taken off: in the latter caſe, as the dry bladder conducted very imperfectly, the fire of the charge ſpread in a beautiful manner, covering a ſpace of about an inch in diameter.

This electrical exploſion would not paſs in the leaſt degree, over the ſurface of new glaſs, notwithſtanding its property of diffuſion above mentioned ſeemed to promiſe that it might. Neither would it paſs at all over the ſurface of alum, rock ſalt, ſal ammoniac, blue or green vitriol, or a piece of poliſhed agate; though theſe are all conductors of a middle kind, like water; and ſeveral of them had very ſmooth ſurfaces. It alſo refuſed the ſurface of dry wood, and dry leather, even the ſmootheſt cover of a book.

But I found that I had concluded too ſoon, that this paſſage of the electrical exploſion was

was peculiar to the ſurface of water, by finding, firſt, that it paſſed over the ſurface of a touch-ſtone, and then over a piece of the beſt kind of iron ore, exceedingly ſmooth on ſome of its ſides. This piece is about an inch thick, and about three inches in its other dimenſions. The full charge of a jar of three ſquare feet would not enter it. It was diverting to obſerve how the electrical exploſion would make a circuit, round its angles, when it was made in a place remote from the jar. It looked like a thing invulnerable.

THIS electrical exploſion paſſed over the ſurface of oil of vitriol with a dull ſound, and a red colour, which was the only appearance of the kind that I have yet met with. In all other caſes, if it paſſed at all, it was in a bright flame, and with a report peculiarly loud It paſſed over the ſurface of the moſt highly rectified ſpirit of wine without firing it; but when I took too great a diſtance, the electric fire entered the ſpirit, and the whole plate was in a blaze in a moment.

I ONCE fancied that the fluidity of water was in a great meaſure the cauſe of this phenomenon; but I found I could not make it paſs over the ſurface of quickſilver or melted lead; though neither of the rods with which the diſcharge was made touched the metals. A dark impreſſion was made on the ſurfaces of both the quickſilver and the lead, of the uſual ſize of the circular ſpot; and re-

mained

mained very viſible, notwithſtanding the ſtate of fuſion in which the metals were.

So far was the electrical exploſion from paſſing over the ſurface of any metal, that I obſerved, if the diſtance through the air, in order to a paſſage through the metal, was ever ſo little nearer than the diſtance along the ſurfaces, it never failed to enter the metal; ſo that its entering the ſurface of the metal, and its coming out again ſeemed to be made without the leaſt obſtruction. If as much water was laid on a ſmooth piece of braſs as could lie upon it, it would not go over the ſurface of the water, but always ſtruck through the water into the metal. But if the metal lay at any conſiderable depth under the water, it would prefer the ſurface. It even paſſed over three or four inches of the ſurface of water as it was boiling in a braſs pot over the fire, in the midſt of the ſteam and the bubbles, which ſeemed to be no hindrance to it.

ANIMAL fluids, of all kinds that I have tried, ſeemed in a peculiar manner to favour the paſſage of the electrical exploſion over their ſurfaces, and the report of thoſe exploſions was manifeſtly louder than when water was uſed in the experiment. This I remarked more particularly when I made uſe of milk, the white and yolk of an egg, both freſh broken, and after it had ſtood a day or two, and had contracted a hard pellicle. In all the experiments with the egg, it was obſerved,

ſerved, that no peculiar impreſſion was made in the place where the electric matter firſt came upon the ſurface.

It was very remarkable, that the report made by all theſe exploſions, in which the electric matter paſſed over the ſurfaces, was conſiderably louder than when the diſcharge was made between two pieces of metal; and they were obſerved by perſons at ſome diſtance out of the houſe, and in a neighbouring houſe, very much to reſemble the ſmart cracking of a whip; and indeed it would not be very eaſy to diſtinguiſh them. But the ſound made by theſe exploſions, though by far the loudeſt that I ever heard of the kind, fell much ſhort of the report made by a ſingle jar, of no very great ſize, of Mr. Rackſtrow's; who ſays that it was as loud as that of a piſtol.

It was pretty evident, that the diſtance at which the fire paſſed over animal ſubſtances was greater than it could be made on the ſurface of water; particularly in the firſt experiment of the leg of mutton. It alſo paſſed about ten inches over the ſurface of a piece of ſpinal marrow taken from an ox.

I was much ſtruck with a beautiful appearance which occurred in the courſe of theſe experiments, though it was of a different nature from them. When the electrical exploſion does not paſs over the ſurface of water, but enters the fluid, it makes a regular ſtar upon it, conſiſting of ten or a dozen rays; and, what is moſt remarkable, thoſe rays which ſtretch towards the braſs rod that communicates with the outſide of the battery are always longer than the reſt; and if the exploſion be made at ſuch a diſtance, as to be very near taking the ſurface, thoſe rays will be four or five times longer than the reſt; and a line bounding the whole appearance will be a beautiful ellipſis, one of whoſe *foci* is perpendicularly under the braſs knob with which the diſcharge is made.

It will be in vain to attempt theſe experiments without a conſiderable force. Nothing at all, to any purpoſe, can be done with a common jar; ſince the exploſion of it will hardly paſs over the ſurface of any conductor farther than it will diſcharge through the air. The charge of a jar containing three ſquare feet of coated glaſs will not make any conſiderable appearance upon the water; and, as far as I can judge, the diſtance at which the exploſion will paſs along any ſurface is in proportion to the ſtrength of the charge. For this reaſon I make no doubt but that I could have performed all the experiments above mentioned to much greater advantage, if I had applied a greater force, but that would have required more time, and a moderate force was ſufficient to aſcertain the facts.

N. B.

N. B. In theſe experiments, I put the diſcharging rod through a handle of baked wood; by which means, I could with ſafety lay one end of it upon the wires of the battery, and make the explosion with the other, on what ſubſtances I pleaſed.

SECTION XII.

EXPERIMENTS ON THE TOURMALIN.

FATIGUED with the inceſſant charging of the electrical battery, and ſtunned with the frequent report of its exploſion, I was deſirous of ſome reſpite from thoſe labours, and with pleaſure took up the gentle and ſilent TOURMALIN. And I make no doubt but that my readers, who muſt have ſympathized with me, will be equally pleaſed with the change.

IT was in the month of Auguſt 1766, that, being in London, I received from Dr. Heberden, who is glad to encourage every attempt in philoſophical inquiries, his ſet of tourmalins; among which was that fine one which had paſſed through the hands of Mr. Wilſon and Mr. Canton, and of which a deſcription is given in the fifty-firſt volume of the Philoſophical Tranſactions, p. 316. But notwithſtanding I had this valuable ſtone ſo long in my poſſeſſion, it was not till the latter end of December that I began to make any experiments with it, having, in the mean time, been engaged in other electrical purſuits. At length, however, having brought my other experiments to the ſtate in which the reader hath ſeen them, I was deſirous of being an eye witneſs of the wonderful properties of this ſtone, and of purſuing a few hints which had occurred to me with reſpect to it. The reſult of my experiments I ſhall lay before the reader, after having informed him in what manner, and with what precautions they were made.

The methods I uſed to apply heat to the tourmalin were various, but they will be ſufficiently explained in the particular experiments. To aſcertain the kind of electricity, I always had near me a *ſtand of baked wood*, from the top of which projected various arms for different purpoſes. Three of them were of glaſs, to two of which were faſtened threads of ſilk, as it comes from the worm, ſupporting light pieces of down; from the other hung a fine thread, about nine or ten inches long; while a braſs arm ſupported a pair of Mr. Canton's pith balls. At the other extremity of this arm, which was pointed, I could place a charged jar, to keep the balls conſtantly and equably diverging, with poſitive or negative electricity. Sometimes I ſuſpended the balls, not inſulated, within the influence of large charged jars. And laſtly, I had always at hand *a fine thread of trial* not inſulated, and hanging freely, to obſerve whether the ſtone was electrical or not when I began any experiments, and ſometimes to meaſure the ſtrength of the power which it had acquired.

Before I began any experiments, I never failed to try how long my electrometers would retain electricity, and in what degree. If the thread would retain the virtue for a few minutes, I generally preferred it, when I wanted to communicate the electricity of the tourmalin, becaufe it would catch it in a moment. If the thread would not retain the virtue long enough, or if I wanted a lefs variable degree of electricity than the thread could retain, I had recourfe to the *feathers*, which never failed to retain the virtue that was communicated to them for feveral hours together. I have often found them pretty ftrongly electrified, after remaining untouched a whole night, though there had been no fire in the room. They might be touched without any fenfible lofs of their electricity; but they received the virtue very flowly.

The reader muft obferve, that by the *pofitive* or *negative* fide of a tourmalin, in the following experiments, I always mean the fide which is pofitive or negative while the ftone is cooling. Alfo, when I mention *the tourmalin* without any diftinction, I always mean Dr. Heberden's large one, the convex fide of which is pofitive in cooling, and the flat fide negative.

The confideration of Mr. Wilcke's experiments on the production of fpontaneous electricity, by melting one fubftance within another, firft made me conjecture, that the tour-

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malin might collect its electricity from the neighbouring air. To aſcertain this circumſtance I made the following experiments, which ſeem to prove that my conjecture was juſt. It was with a view to this experiment that I firſt expreſſed a deſire to have a tourmalin in my poſſeſſion. I afterwards found that Mr. Wilſon had made an experiment, mentioned vol. I. p. 372, which is, in part, favourable to this hypotheſis, though he ſuppoſed the electricity to permeate the ſtone, ſo that one ſide might have been ſupplied from the other. But the following experiments will ſhow, that the ſuppoſition of the permeability of the tourmalin to the electric fluid is altogether unneceſſary to account for any of the appearances it exhibits.

On the ſtandard bar of a moſt excellent pyrometer made by Mr. Ellicott, I laid a part of a pane of glaſs, and upon the glaſs Dr. Heberden's large tourmalin. The bar was heated by a ſpirit lamp placed underneath it; and I treated the tourmalin in this manner, to aſcertain with exactneſs when the heat was increaſing, decreaſing, or ſtationary. In this diſpoſition of my apparatus I obſerved, that, whenever I examined the tourmalin, the glaſs had acquired an electricity oppoſite to that of the ſide of the ſtone which had lain upon it, and equally ſtrong. If, for inſtance, I preſented the flat ſide of the ſtone to a feather electrified poſitively, as the heat was increaſing, it would repel it at the diſtance of about two inches, and the glaſs would attract it at the ſame, or a greater diſtance; and when the heat was decreaſing, the ſtone would attract it, and the glaſs repel it at the diſtance of four or five inches. It made no difference which ſide of the glaſs I preſented; both ſides attracting or repelling the ſame feather with equal ſtrength. When I faſtened a ſhilling with ſealing-wax upon the glaſs, the events were always the ſame. The electricity of both the ſhilling and the glaſs was always oppoſite to that of the ſtone. I was ſurpriſed to obſerve how ſoon the electricity, both of the ſtone and the glaſs, would change when it came to the turn; for in leſs than a minute I have ſometimes found them the reverſe of what they were before.

There was, however, in the caſes in which I laid the convex ſide of the tourmalin upon the flat ſurface of the glaſs, or ſhilling, one exception to the rule above mentioned, viz. that, in cooling, the glaſs and ſhilling were poſitive, as well as the ſtone. This I imagined to be owing to the ſtone touching the ſurface on which it lay in ſo few points, that it collected its electricity from the air, and imparted it to the body on which it lay; and this ſuppoſition was confirmed by experiment. For getting a mold made for the convex ſide of the ſtone in plaiſter of Paris, and heating the tourmalin in the mold, faſtened to a ſlip of glaſs, I always found the mold and the glaſs poſſeſſed of the electricity contrary to that of the ſtone, and equally ſtrong. When they were cooling, the mold ſeemed ſome-

ſometimes to be more ſtrongly negative than the ſtone was poſitive; for, at one time, when the ſtone repelled the thread at the diſtance of about three inches, the mold attracted it at the diſtance of near ſix.

Having made the experiments above mentioned with the tourmalin placed upon glaſs, or conducting ſubſtances, laid upon the glaſs, I had the curioſity to try what would be the conſequence of heating and cooling the ſtone in contact with other ſubſtances, both electrics and conductors. And theſe experiments brought me gradually to the diſcovery of a method of reverſing all the experiments that have hitherto been made upon the tourmalin, making that ſide which is poſitive in heating or cooling to be negative, and that which is negative to be poſitive; ſo that the kind of electricity ſhall be juſt what the operator ſhall direct, by the application of proper ſubſtances to the ſtone.

I began theſe experiments with ſubſtituting another tourmalin inſtead of the piece of glaſs above mentioned; and when only one of the tourmalins was heated, they were both affected juſt as the tourmalin and glaſs had been. If, for inſtance, the negative ſide of a hot tourmalin was laid upon the negative ſide of a cold one, this latter became poſitive, as a piece of glaſs would have been in the ſame circumſtances.

When I heated both the tourmalins, though they were faſtened together with cement, they both acquired the ſame power that they would

would have done in the open air. In theſe caſes, as the ſtones could not be made to touch one another in a ſufficient number of points, nothing could be concluded from the experiments. The ſame objection lay againſt heating or cooling the tourmalin upon rough glaſs; when I always found them both to be affected as they would have been if the glaſs had been ſmooth.

THIS conſideration made me think of cooling the tourmalin in contact with *ſealing-wax*, which might be made to fit the ſtone as exactly as poſſible, though it were ever ſo irregular. Accordingly I half buried the negative ſide of a tourmalin in hot ſealing-wax; and when it was cold, turning it out of its waxen cell, found it poſitive (contrary to what it would have been in the open air) and the wax negative. The other ſide of the tourmalin, which was expoſed to the open air, was affected in the ſame manner as it would have been if the oppoſite ſide had been expoſed to the air too, ſo that both ſides were poſitive in cooling. As the negative ſide of the tourmalin became poſitive by cooling in wax, I had no doubt but that the poſitive ſide would be ſo, as I actually found it.

I WOULD have aſcertained the ſtate of the different ſides of the tourmalin when it was *heating* in wax, but I found it extremely difficult to do it with ſufficient certainty. It cannot be known exactly when the ſtone begins to cool in theſe circumſtances; beſides, in this method of treatment, it muſt neceſſarily ly be ſome time in the open air before it can be preſented to the electrometer; and the electricity of the ſides in heating is by no means ſo remarkable as it is in cooling. In the attempts I did make with the poſitive ſide of the tourmalin buried in wax, I generally found it negative, but once or twice it ſeemed to be poſitive.

WHEN I cooled the tourmalin in *quickſilver*, contained in a china cup, it always came out poſitive, and left the quickſilver negative; but this effect could not be concluded to be the conſequence of the application of the one to the other, becauſe it is almoſt impoſſible to touch quickſilver with the tourmalin without ſome degree of friction; which never fails to make both ſides ſtrongly poſitive though it be quite cold, and eſpecially if the ſtone be dipped deep into it.

IT then occurred to me, that the tourmalin would not be apt to receive any friction from ſimple preſſure againſt the palm of my hand; and this being a conducting ſubſtance communicating with the earth, the circumſtances of the experiment would be new, and might poſſibly produce new appearances. The event more than anſwered my expectations: for in heating or cooling the tourmalin in contact with the palm of my hand, each ſide of the ſtone was affected exactly in a manner contrary to what it would have been if expoſed to the open air. In this caſe, though the poſitive appearances may be ſuſpected to be ambiguous, on account of the difficulty of avoid-

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ing ſome ſmall degree of friction, in removing the ſtone from the hand; yet the negative appearances are, by that very circumſtance, rendered the more indiſputable, and therefore remove the objection from the poſitive ones. For the greater ſatisfaction of my reader, I ſhall relate theſe experiments exactly as they were made.

I FASTENED the convex ſide of Dr. Heberden's large tourmalin to the end of a ſtick of ſealing-wax, and when it was quite cold, I preſſed the flat ſide of it pretty hard againſt the ſofteſt part of the palm of my hand. Immediately upon this, preſenting it to an electrified feather, it appeared to be ſtrongly negative, contrary to what it would have been if expoſed to the open air; and it continued negative till it had acquired all the heat it could get from my hand, when its power decreaſed, though it was ſenſibly negative to the laſt. Perceiving no alteration, I let the ſtone cool in the open air; when, according to Mr. Canton's rule, it grew more ſtrongly negative, till it was quite cold. Thus the ſame ſide of the ſtone was made negative both in heating and cooling.

HEATING the ſame flat ſide, by holding it near a red-hot poker, and then juſt touching it with the palm of my hand (when I could not bear it to reſt a moment) it became poſitive. Letting it cool in the air, it was negative, and touching it again with my hand it became poſitive. Thus I made the ſame ſide of the ſtone alternately poſitive and negative

tive for a confiderable time; and at length, when I could bear to keep it upon my hand, it acquired a ftrong pofitive electricity, which continued till it was brought to the heat of my hand.

To complete thefe experiments, I removed the wax from the convex fide, and faftened it to the flat fide of the ftone. Then warming the convex fide, by preffing it againft the palm of my hand, it became pretty ftrongly pofitive, contrary to what it would have been if heated in the open air, and continued pofitive in a fmall degree after it had got all the heat it could from my hand. Letting it cool in the open air, it grew, according to Mr. Canton's rule, more ftrongly pofitive, and continued fo till it was quite cold. Thus the fame fide of the ftone was made pofitive both in heating and cooling.

I THEN heated the convex fide, by holding it near a red-hot poker, and preffing it againft the palm of my hand, as foon as I could bear it, it became (contrary to what it would have been in the open air) pretty ftrongly negative; though it be extremely difficult to get a negative appearance from this fide. It cannot always be catched when it is heating in the open air. Care, however, muft be taken, left a flight attraction of the electrified feather, by a body not electrified, be miftaken for negative electricity.

HAVING made the above mentioned experiments, to fee how the tourmalin would be affected by being heated or cooled in contact

tact with various subftances, to which only one of its fides was expofed at once; I made others in which the ftone was entirely furrounded by them. It appeared very evident, from Mr. Canton's experiment, that it could anfwer no purpofe to inclofe it in fubftances that were conductors: for though the two electricities fhould be generated, the equilibrium would inftantly be reftored between them. I therefore made ufe of electric fubftances only, and began with *oil* and *tallow*, both covering the tourmalin with them when it was hot, and alfo heating it in boiling oil. But this treatment produced no new appearance, the electricity of the ftone being only a little leffened. The event was the fame when a tourmalin was covered with *cement* made of bees wax and turpentine.

At laft I made a fmall tourmalin very hot, and dropping melted fealing-wax upon it, covered the ftone all over, to the thicknefs of about a crown piece; and found it to act nearly, if not quite as well through this coating of wax, as if it had been expofed to the open air. I take it for granted, that the infide of the cafe of wax next to the ftone was poffeffed of the electricity oppofite to that of the ftone, at the fame time that the outfide was the fame with it. A pretty deception may be made by means of this experiment; for if a tourmalin be concealed in a ftick of fealing-wax, the wax will feem to have acquired the properties of the tourmalin.

Heating the ſtone, or letting it cool *in vacuo* might eaſily be imagined to have the ſame effect as heating or cooling it in contact with conducting ſubſtances; I had the curioſity, however, to try the experiment, by letting it cool in an exhauſted receiver, in which I had a contrivance to bring a thread of trial near it, or withdraw it at pleaſure. The ſtone was ſet upright on its edge, by means of bits of glaſs which it touched but in a few points. The conſequence was, that the virtue of the ſtone ſeemed to be diminiſhed about one half; owing, perhaps, to the vacuum not being ſufficiently perfect. For the ſame reaſon, the tourmalin has but little virtue immediately upon being taken out of boiling water, or after being heated in flame.

One time I fixed a thin piece of glaſs, with a ſmall coating upon it, oppoſite and parallel to the flat ſide of the tourmalin, and at about a quarter of an inch diſtance from it, in an exhauſted receiver; to obſerve whether the electricity would be tranſmitted from the glaſs to the ſtone through the vacuum: but though the glaſs was electrified, it was ſo ſlightly, that I could not be certain of what kind it was.

In order to aſcertain the circumſtances relating to the change of the electricity of the tourmalin with more exactneſs than could be done by heating and cooling the ſtone in any of the uſual methods, I laid it upon the ſtandard bar of the pyrometer, and communicated heat

heat to it by a ſpirit lamp placed underneath it. The reſult of theſe experiments was in general agreeable to Mr. Canton's rules; but a few circumſtances occurred in this method of treating it, which could not be determined in any other; and therefore it may be worth while juſt to mention them. I generally heated the bar, which is of iron, eight inches long, till the index moved ſeventy degrees, each of which correſponds to one 7200th part of an inch; and obſerved, that which ever ſide of the ſtone lay uppermoſt, it was extremely difficult to aſcertain the nature of its electricity all the time the heat was increaſing; though, in order to do it, I held over it an electrified thread, about two inches in length, faſtened to a ſtick of ſealing-wax, which juſt ſupported it in an horizontal ſituation. It was evident, however, that it was electrified, by its attracting a thread of trial at the diſtance of about a quarter of an inch; but if I took the ſtone off the bar, and immediately preſented the ſide that had lain upon it to an electrified thread or feather, I always perceived the convex ſide to be negative, and the flat ſide poſitive in the ſame circumſtances; but not half ſo much as they were in the contrary ſtate by cooling. In this caſe, the two powers were very diſtinguiſhable by the ſmall thread above mentioned, as the ſtone lay upon the bar; and alſo by bits of down faſtened to ſilk threads. One of theſe, which had touched the convex ſide of the ſtone, as it lay uppermoſt upon the bar, could not

not be made to touch it again in lefs then five hours and a half.

To fee what would be the effect of keeping the tourmalin in the very fame degree of heat a confiderable time together, I laid it upon the middle part of the bar, heated by two fpirit lamps, one at each extremity, and making the index move forty-five degrees, I kept it in the fame degree of heat, without the leaft fenfible variation, for above half an hour together; and obferved, that the upper fide, which was the convex one, was always electrified to a fmall degree, attracting a fine thread at the diftance of about a quarter of an inch. If in that time I took it off the bar ever fo quick, and prefented it to an electrified feather; the flat fide, which lay upon the bar, was negative, and the upper fide very flightly pofitive; as appeared by its only not attracting the feather. When I put a piece of glafs betwixt the ftandard bar and the tourmalin, and kept them likewife in the fame degree of heat, for the fame fpace of time, the refult was the fame as before, and the glafs was flightly electrified, in a kind oppofite to that of the ftone.

In heating the tourmalin upon the pyrometer, one of its fides was neceffarily made much hotter than the other. This inconvenience I avoided in the following method of treatment, which, though not fo accurate in fome refpects, has peculiar advantages in others. By means of two rough places in the ftone, I tied it in a filk thread, which only touched the extreme edge of it on both ſides. Being in this manner perfectly inſulated, I contrived to make it hang in the air, at any diſtance from a fire, or candle, &c. and by twiſting the ſtring, I could make it preſent both its ſides alternately, ſo as to heat it very equally.

When, in this manner, I had made it ſo hot, that I could hardly bear to handle it, I let it remain in the ſame ſituation a quarter of an hour, in order to be ſure that it was heated equally throughout. Then, with a bundle of fine thread, held ſome time before in the ſame degree of heat, I took off the electricity which the ſtone had acquired in heating, and continuing it in the ſame ſituation, I found it acquired extremely little, if any electricity. Sometimes, when I thought it had acquired a little (which might be occaſioned by the variation of heat in the fire) it was ſo ſmall, that I could not determine of what kind it was. This fully ſatisfied me of the juſtneſs of Mr. Canton's obſervation that it is not heat, but the circumſtance of changing its degree of heat that gives electricity to this ſtone.

If the ſtone be heated pretty ſuddenly, I have ſometimes found that it may be handled, and preſſed with the fingers ſeveral times before the electricity it acquires in heating will be changed, though it begins to cool the moment it is removed from the fire.

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In this ſame method of treatment, I verified Mr. Canton's obſervation, that when the tourmalin is heated, and ſuffered to cool again, without either of its ſides being touched, the ſame ſide will be poſitive or negative the whole time of the increaſe and decreaſe of the heat. But, as he obſerves, in his experiments on hot air, the ſtone muſt, in this caſe, be heated only to a ſmall degree. I alſo proved the converſe of this propoſition; for, beginning where I left the ſtone in the laſt experiment, and removing it farther from the fire, both ſides acquired a ſtrong electricity, as uſual; and bringing it again nearer to the fire, I obſerved that both the ſides not only retained the electricity they had acquired in cooling, all the time it was heating, but a conſiderable time after it had remained in the ſame degree of heat.

I cannot, however, entirely acquieſce in the reaſon that Mr. Canton gives for this appearance: for if the ſurrounding air would conduct the electric fluid from the poſitive ſide of the ſtone to the negative, I ſhould think it would be in the ſame ſituation as in the experiment Mr. Canton made upon it ſurrounded with water, and that neither ſide would diſcover any electricity at all. When the heat is three or four times greater than is ſufficient to change the electricity of the two ſides, the virtue of the ſtone is the ſtrongeſt, and appears to be ſo when it is tried in the very neighbourhood of the fire. In the very center of the fire, the ſtone never fails to cover itſelf with aſhes, attracted to it from all ſides, and from this property it acquired its name in Dutch.

It requires, indeed, ſome time for the electricity of the ſides to change from one ſtate to the other; and therefore the time of the ſenſible change is not always at the time of its beginning to cool, but theſe two circumſtances will be brought nearer together the hotter the ſtone is made, becauſe then the efforts (of whatever kind they are) to acquire any particular ſpecies of electricity will be the moſt vigorous, and ſooner produce their effect; ſo as to be more able to overcome obſtacles to it, ſuch as muſt ariſe from the contrary electricity with which the ſtone is poſſeſſed. Thus, if either ſide of the ſtone be in a ſtate to acquire either kind of electricity, and a quantity of the contrary electricity be communicated to it by friction or *ab extra*, that foreign electricity will be either only weakened, or loſt, or changed; and theſe in a longer or a ſhorter ſpace of time, according to the vigour, as we may ſay, with which the ſtone is made to exert itſelf to counteract that influence. But I have great reaſon to ſuſpect my own opinion, when it is different from that of ſo accurate and excellent a judge of this ſubject as Mr. Canton.

It is a fact, however, that the ſtone often changes its electricity very ſlowly; and the elec-

electricity it acquires in cooling never fails to remain many hours upon it, with very little diminution. It is even poſſible that, in ſome caſes, the electricity acquired by heating may be ſo ſtrong, as to overpower that which is acquired by cooling; ſo that both ſides may ſhow the ſame power in the whole operation. And I am very certain that, in my hands, both the ſides of Dr. Heberden's large tourmalin have frequently been poſitive for ſeveral hours together, without any appearance of either of them having been negative at all. Perhaps the flat ſide of this ſtone, which is poſitive in heating, might continue ſo according to Mr. Canton's obſervation, and the electricity of the convex ſide might have changed, as it very often does, too ſoon for me to obſerve it. This fact, however, has happened ſo often with me, and is ſo very remarkable, that I think I ought not to omit the mention of it, let the cauſe be what it will.

THIS appearance happened ſo conſtantly when I firſt began to make experiments with the tourmalin, that I had concluded the Duc de Noya had reaſon to aſſert, contrary to Æpinus, that both ſides of the tourmalin in all caſes acquired poſitive electricity; and I ſhould have acquieſced in that opinion, had it not been for the friendly remonſtrances of Dr. Franklin and Mr. Canton; in conſequence of which I renewed my experiments, and at length found other appearances. At the time above mentioned, I generally heated the tourmalin by preſenting each ſide alternately to a red-hot poker, or a piece of hot glaſs held at the diſtance of about half an inch; and ſometimes I held it in the focus of a burning mirror ; but I have ſince found the ſame appearance when I have heated it in the middle of an iron hoop made red-hot. The ſtone, in all theſe caſes, was faſtened by its edge to a ſtick of ſealing-wax. This appearance I have obſerved to happen the ofteneſt when the iron hoop has been exceedingly hot, ſo that the outſide of the ſtone muſt have been heated ſome time before the inſide; and I alſo think there is the greateſt chance of producing this appearance when the convex ſide of the ſtone is made the hotter of the two. When I heat the large tourmalin in this manner, I ſeldom fail to make both ſides poſitive till the ſtone be about blood warm. I then generally obſerve a ragged part of the flat ſide, towards one end of the ſtone, will become negative firſt, and by degrees the reſt of the flat ſide; but very often one part of the flat ſide will, in this method of treatment, be ſtrongly poſitive half an hour after the other part is become negative.

This account of the appearance is made the more probable by the manner in which the ſtone was affected when only one of its ſides was heated at one time. For when the convex ſide only was heated, the ſtone often con-

continued a long time with both its ſides poſitive, generally till it was not ſenſibly warm. But, in this caſe, before the convex ſide became poſitive, it would ſometimes be negative two or three minutes. On the other hand, when the flat ſide only was heated, it would be poſitive a long while, and the convex ſide negative; but the flat ſide becoming negative a conſiderable time before the convex ſide ceaſed to be ſo, both ſides would continue negative till the ſtone was nearly cold.

EXTREMELY ſorry I am for the article with which I muſt cloſe this ſection. In the firſt of the above mentioned courſes of experiments, that fine tourmalin, which has been ſo often mentioned in the courſe of this work, ſlipped out of my hands; and though it fell only from the height of my breaſt, upon a boarded floor, two pieces were broke off from one of its ends. The ſtone, however, is more disfigured than injured by the accident: for the larger of the fragments weighs but ten grains, and the ſmaller only one, while the reſt of it weighs four penny weights ſixteen grains. I cannot perceive that its virtue is at all leſſened. Mr. Wilſon obſerves, that there were ſeveral cracks in it; and for that reaſon I had been careful never to expoſe it to any great degree of heat.

IT is broke with eight or ten different faces, each of which hath a moſt exquiſite poliſh; but there is no appearance of any *ſtrata* or *laminæ* in the internal ſtructure of the ſtone. A piece of glaſs or pitch might be ſuppoſed to break in the ſame manner. The larger of the fragments has conſiderable power, and the two ſides have the ſame different powers that they had when they were part of the intire ſtone.

SECTION XIII.

Experiments in which RINGS, consisting of all the PRISMATIC COLOURS, were made by electrical explosions on the surfaces of metals.

IT was a diſcovery of Sir Iſaac Newton, that the colours of bodies depend upon the thickneſs of the fine plates which compoſe their ſurfaces. He hath ſhown that a change of the thickneſs of theſe plates occaſions a change in the colour of the body, rays of a different colour being thereby diſpoſed to be tranſmitted through it; and, conſequently, rays of a different colour reflected at the ſame place, ſo as to preſent an image of a different colour to the eye. A variation in the denſity occaſions a variation in the colour, but ſtill a medium of any denſity will exhibit all the colours, according to the thickneſs of it. Theſe obſervations he confirmed by experiments on plates of air, water, and glaſs. He likewiſe mentions the colours which ariſe on poliſhed ſteel by heating it, as likewiſe on bell metal, and ſome other metalline ſubſtances, when melted and poured on the ground, where they may cool in the open air, and he aſcribes them to the

ſcoriæ,

ſcoriæ or vitrified parts of the metal, which he ſays moſt metals, when heated or melted, do continually protrude, and ſend out to their ſurfaces, covering them in the form of a thin glaſſy ſkin.

THIS great diſcovery concerning the colours of bodies, depending upon the thickneſs of the fine plates which compoſe their ſurfaces, of whatever denſity thoſe plates may be, I have been ſo happy as to hit upon a method of illuſtrating and confirming by means of electrical exploſions. A number of theſe being received on the ſurface of any piece of metal, change the colour of it to a conſiderable diſtance from the ſpot on which they were diſcharged, ſo that the whole circular ſpace is divided into a number of concentric rings, each of which conſiſts of all the priſmatic colours, and perhaps as vivid as they can be produced in any method whatever.

IT was not by any reaſoning *a priori*, but by mere accident, that I was led to the diſcovery of theſe colours. Having occaſion to take a great number of exploſions, in order to aſcertain the lateral force of them; I obſerved that a piece of braſs, through which they were tranſmitted, was not only melted, and marked with a circle by a fuſion round the central ſpot, but likewiſe tinged beyond the circular ſpot with a greeniſh colour, which I could not eaſily wipe out with my finger. Struck with this new appearance, I replaced the apparatus, and continued the exploſions, till,

till, examining with a microſcope, I plainly perceived all the priſmatic colours, in the order of the rainbow. The diameter of the red, in this inſtance, was one third of an inch, and of the purple near one fourth. The diameter of the whole coloured ſpace in the ſubſequent experiments, in which I generally uſed thirty or forty exploſions, was near an inch.

PLEASED with the firſt experiment, I preſently diverſified it in a great variety of ways, the reſult of which I ſhall compriſe in the following obſervations.

1. WHEN a pointed body is fixed oppoſite to a plain ſurface, the nearer it is placed, the ſooner the colours appear, the cloſer do they ſucceed one another, and the leſs ſpace they occupy. It ſeems, however, that when the point is at ſuch a diſtance, that the electric matter has room to expand, and form as large a circular ſpot as the battery will admit, this coloured ſpace is as large as it is capable of being made; but ſtill the colours appear later, in proportion to the diſtance beyond that. When the point is fixed exceedingly near, or made to touch the ſurface, the colours appear at the firſt exploſion, but they ſpread very irregularly, and make no diſtinct rings.

2. THE more accutely pointed is the wire, from which the electric fire iſſues, or at which it enters, the greater is the number of rings. A blunt point makes the rings larger, but

but fewer. It is likewiſe much later before they make their appearance at a given diſtance.

3. In making theſe rings, the firſt appearance is a duſky red about the edge of the central ſpot; preſently after which (generally after four or five ſtrokes) there appears a circular ſpace, viſible only in an oblique poſition to the light, and looking like a ſhade on the metal. This expands very little during the whole courſe of the exploſions. It ſeems to be an attempt, as it were, at the firſt red; for, by degrees, as the other colours fill the greater part of that ſpace, the extreme edge of it becomes a deeper brown.

4. After a few more exploſions, a ſecond circular ſpace is marked, by another ſhade beyond the firſt, being one eighth or one tenth of an inch in breadth, which I have never obſerved to change its appearance, after ever ſo many exploſions. This ſhade, by ſucceeding the firſt, which becomes gradually of a brown or light red colour, ſeems to be an attempt at the fainter colours, which intervene between the reds.

5. All the colours make their firſt appearance about the edges of the circular ſpot. More exploſions make them expand towards the extremity of the ſpace firſt marked out; while others ſucceed in their place; till, after thirty or forty exploſions, three diſtinct rings appear, each conſiſting of all the colours. If the

the explofions be continued farther, the colours become lefs beautiful and diftinct, the red generally prevailing, and fuffufing the reft.

6. The innermoft, *i. e.* the laft formed colours, are always the moft vivid, and thofe rings are likewife clofer to one another than the reft.

7. These colours may be brufhed with a feather, or a finger may be drawn over them, without injury; but they are eafily peeled off, with one's nail, or any thing that is fharp. The innermoft are the moft difficult to erafe.

8. The firft rings are fometimes covered with a quantity of black duft, part of which, however, may be wiped off, with a feather, and the colours will appear under it.

9. It makes no difference whether the electric matter iffue from the pointed body upon the plate, or from the plate upon the pointed body, the furface oppofite to the point being marked exactly alike in both cafes; alfo the points themfelves, from which the fire iffues, or at which it enters, are coloured to a confiderable degree, about half an inch. The colours, alfo, return here as upon the plate.

10. The more circles are made at the fame time, the more delicate, I think, the colours will be, whereas the furface is torn, as it were, by violent explofions, and the colours appear rough and coarfe. But this roughnefs is only perceived on fteel. On filver,

tin, and polifhed brafs, the colours were always equally free from that coarfenefs.

11. A POLISHED furface is not neceffary for thefe colours, for they appear very well, though they do not make fo beautiful an appearance on the rough furfaces.

12. THESE coloured rings appear equally well on all the metals that I have tried, viz. gold, filver, copper, brafs, iron, fteel, lead, and tin. I have not tried any of the femi-metals, but I have no doubt of their anfwering as well as the proper metals.

13. WHEN the pointed wire was made to incline to the plane on which the colours were made, the circular fpot was quite round, and the center of it was in a perpendicular let fall from the point upon the plain furface; but the colours were projected in an oblong form, the center being in the pointed wire continued.

UPON fhewing thefe coloured rings to Mr. Canton, I was agreeably furprized to find, that he had likewife produced all the prifmatic colours from all the metals, but by a different operation. He extended fine wires, of all the metals, along the furface of pieces of glafs, ivory, wood, &c. and when the wire was exploded, he always found them tinged with all the colours. They are not difpofed in fo regular and beautiful a manner as in the rings I produced, but they equally demonftrate, that none of the metals, thus exploded, difcovers the leaft preference to one colour more than to another. A variety of other very extraordinary

 appear-

appearances occurred in the courfe of Mr. Canton's experiments in melting wires, but I forbear to mention them, as I hope he will foon favour the public with a communication of them himfelf.

In what manner thefe colours are formed, it may not be eafy to conjecture. In Mr. Canton's method of producing them, the metal, or the calcined and vitrified parts of it, feem to be difperfed in all directions from the place of explofion in the form of fpheres, of a very great variety of fizes, tinged with all the variety of colours, and fome of them fmaller than can be diftinctly feen by any magnifier. In my method of making thefe colours, they feem to be produced in a manner fimilar to the production of colours on fteel, and other metals by heat, *i. e.* the furface is affected, without the parts of it being removed from their places, certain plates or *laminæ* being formed, of a thicknefs proper to exhibit the refpective colours.

SECTION

SECTION XIV.

Experiments on the LATERAL FORCE of ELECTRICAL EXPLOSIONS.

BEING informed, in accounts of damages done by lightning, of perſons and things being removed to a conſiderable diſtance, without receiving any hurt; I was excited to try whether I could produce ſimilar effects by electricity. All the other known effects of lightning had been frequently imitated by the application of this power, but I do not know that this effect has ever been ſo much as taken notice of by any electricians. The experiments I preſently found to be very eaſy, and I think it not difficult to aſcertain the cauſe, and the manner in which this ſtriking effect is produced.

If pieces of cork, powder of any kind, or any light bodies whatever, be placed near the exploſion of a jar or battery, they will not fail to be moved out of their places, upon the inſtant of the diſcharge. If the exploſion of a large battery be made to paſs over the ſurface of animal or vegetable ſubſtances, in the manner deſcribed above, and large corks be ſtrewed along or near the part intended for it, it is ſurprizing to obſerve with what violence they

they will be driven about the room. This difperfion is in all directions from the center of the explofion, and it makes no difference whether the rods, between which it is made, be fharp pointed or otherwife.

THE effect of this lateral force is very remarkable in attempts to fire gun powder in electrical explofions. If the gun powder be confined ever fo clofe in quills or cartridges, and they be held faft in vifes; yet, when the explofion is made in the center of them, it will fometimes happen (even when a wire has been melted in the midft of the powder, and the fragments have been feen red-hot, for fome time, in different parts of the room) that the powder has not been fired, or only a few grains of it, the reft being difperfed with great violence, part of it flying againft the faces of perfons who affifted in making the experiments. This circumftance, together with the charcoal being a conductor of electricity makes it fo extremely difficult to fire gun powder by electrical explofions; and it is evidently owing to this lateral force, that parts of the melted wire fly fo many ways, and to fo great a diftance from the place of explofion.

THIS lateral force is exerted, not only in the neighbourhood of an explofion, when it is made between pieces of metal in the open air, but alfo when it is tranfmitted through wires that are not thick enough to conduct it perfectly; and the fmaller the wire, and the more complete the fufion, the greater is the difperfion of light bodies placed near it. At one time, when the wire was not melted, but turned blue by the explofion (in which cafe it generally affumes a dufky red, which lafts but for a moment) there was a fmall difperfion from every part of the wire, but by no means fo great as it would have been if it had been melted, or only heated to a greater degree.

By a confiderable number of trials, I found, that a greater force of explofion would move light bodies at a greater diftance, but the fmaller the bodies were, the lefs was this difference; fo that I imagined, that if they had no weight at all, they would, probably, be moved at the fame diftance, by the explofion from any quantity of coated furface, charged equally high: but there was a great difference in the weight removed by different forces at the fame diftance. Placing the fame piece of cork at the fame diftance from the place of explofion, I found that the difcharge of one jar removed it one fourth of an inch, two jars on inch and one fourth, three jars one inch and three fourths, and four about two inches, fo that I do not wonder at very heavy bodies being moved from their places, and to confiderable diftances, by ftrong flafhes of lightning.

That the immediate caufe of this difperfion of bodies in the neighbourhood of electrical explofions is not their being fuddenly charged with a quantity of electric matter, and therefore flying from others that are equally

charged

charged with it is, I think, evident from the following experiments and obſervations. I never obſerved the leaſt ſenſible attraction of theſe light bodies to the braſs rods through which the exploſion paſſed, or to the electric matter paſſing between them, previous to this repulſion, though I uſed ſeveral methods, which could not have failed to ſhow it, if there had been any ſuch thing. Sometimes I ſuſpended them in fine ſilken ſtrings, and obſerved that they had contracted no electricity after they had been agitated in the manner deſcribed above. Sometimes I dipped them in turpentine, and obſerved that no part of it was found ſticking, either to the braſs rods themſelves, or to any part of the table betwixt them and the place where the light bodies had been laid. I even found that the exploſion of a battery, made ever ſo near to a braſs rod, did not ſo much as diſturb the equilibrium of the electric fluid in the body itſelf; for when I had inſulated the rod, and hung a pair of pith balls on the end oppoſite to that near which the exploſion paſſed, I found that the balls were not in the leaſt moved at the time of exploſion; which they would have been, if part of the electric fluid natural to the body had been driven, though but for a moment, towards the oppoſite end. I alſo obſerved that the effect was the ſame when the exploſion was made to paſs through one of the knobs of the inſulated rod. This lateral force was evident through thin ſubſtances of various kinds, interpoſed between the explosion and the bodies removed by it; as paper, tinfoil, and even glass; for when some grains of gun powder were put into a thin phial close stopped, and held near the explosion of a battery, they were thrown into manifest agitation.

I THEREFORE think it most probable, that this lateral force is produced by the explosion of the air from the place where the explosion is made. For the electric matter makes a vacuum of air in its passage; and this air, being displaced suddenly, gives a concussion to all the bodies that happened to be near it. Hence the removal of the light bodies, and the agitation communicated to the thin substances, and to the air, and the light bodies placed beyond them.

THE only objection to this hypothesis is, that this lateral force is not so much less in vacuo as might be expected, when the air is supposed to receive the concussion first, and to communicate it to other bodies; but it must be considered, that the most perfect vacuum we can make with a pump is not free from air. I have tried to make this experiment in a Torricellian vacuum, but could not succeed at that time. Besides, as the electric matter of which an explosion consists must take a wider path in vacuo, if not equally fill the whole space, it may affect a body in its passage, without the intervention of any air. In condensed air, this latter force was not, as far as I could perceive, much encreased.

WILLING

Willing to feel what kind of an impulſe it was that acted upon bodies, when they were driven away by this lateral force of electricity, I held my finger near the path of an exploſion of the battery, paſſing over the ſurface of a green leaf, when I felt a ſtroke, as of ſomething puſhing againſt my finger. Several corks, placed in the ſame ſituation, were driven to a conſiderable diſtance by the ſame exploſion.

Recollecting that this power, which I now call the *lateral force of electrical exploſions*, muſt be the ſame with that which gives the concuſſion to water, mentioned in my experiments to imitate an earthquake, and to vegetable and animal ſubſtances, over the ſurface of which it paſſes; and being determined to make a more ſatisfactory trial of it than I had ventured to do before, I laid a green leaf upon the palm of my hand, intending to make the exploſion paſs over the leaf; but the leaf was burſt, and torn to pieces, and the exploſion paſſing over my hand gave it a violent jar, the effect of which remained in a kind of tingling for ſome time.

Lastly, in order to judge the more perfectly of this force, I laid a chain communicating with the outſide of the battery upon my bare arm above the wriſt, and bringing the diſcharging rod near the fleſh, within about two inches and an half of the chain, I made the exploſion paſs over that quantity of the ſurface of the ſkin. Had I taken a greater diſtance, I was aware that the exploſion would have entered the fleſh, which, I was ſenſible, would have given a painful convulſion to the muſcles through which it paſſed. In this caſe, the ſenſible effect was very different from that, being the ſame external *concuſſion* as before, and I have ſometimes thought, that the ſenſation is not diſagreeable. However the hairs upon the ſkin were all ſinged, and curled up along the whole path of the exploſion, and for the ſpace of about half an inch on each ſide of it alſo the *papillæ pyramidales* of the ſkin were raiſed, as when a perſon is ſhivering with cold. This was alſo the caſe in every part of the arm which the chain touched, and even that part of it which was not in the circuit. Both the path of the exploſion, and the place on which the chain had lain, had a redneſs, which remained till the next day. Sometimes the fleſh has contracted a blackneſs by this experiment, which has remained for a few hours.

SECTION

SECTION XV.

VARIOUS EXPERIMENTS ON THE FORCE OF ELECTRICAL EXPLOSIONS.

MAKING the exploſion of a battery paſs over the ſurface of a green cabbage leaf, I obſerved that it left a track, near a quarter of an inch in breadth, exceedingly well defined, and diſtinguiſhable by a difference of colour from the reſt of the leaf. Along this path alſo the firmneſs of texture in the leaf was entirely deſtroyed, that part becoming quite flexible, like a piece of cloth. Preſently after it turned yellow, grew withered, and became perfectly brittle.

WILLING to try the effect of this exploſion paſſing along the ſurface of other ſubſtances, I laid a piece of common window glaſs on the path, preſſed by a weight of ſix ounces; but it was ſhattered to pieces, and totally diſperſed, together with the leaf on which it lay. Placing the black ſide of a piece of cork wood upon it, preſſed by a weight of half a pound, the leaf was not rent, but the cork was furrowed all the way, a trench being made in it, about half an inch in breadth, and a quarter of an inch in depth. Laying the ſmooth cut ſurface of the piece of cork, it was furrowed all the way

 as

as if it had been cut with a file, but not near so deep as before. Many of the ſmall pieces which had been rubbed off in the exploſion, remained in the furrow. Alſo the ſubſtance of the cork ſeemed to be ſhattered, and it was eaſily rubbed off, a little way into it.

I MADE this exploſion on the ſurface of ſome red wine in a ſmall diſh, and kept a part of the ſame quantity expoſed in a ſimilar manner, but I could perceive no difference between them after ſeveral days.

THE track of an electrical exploſion on the ſurface of the cabbage leaf being ſo well defined, ſuggeſted an experiment to aſcertain whether there was any ſenſible *momentum* in the electric fluid, when it is ruſhing with violence from one ſide of a battery to the other. For this purpoſe I made the exploſion paſs over the leaves when they were cut in right and acute angles; ſo that the ſhorteſt path, from the inſide to the outſide of the battery, was to turn cloſe at the angle; and obſerved, that it was not diverted from its courſe in the leaſt degree by the rapidity of its own motion, but that it had turned exactly at the angle; and kept as cloſe to the oppoſite ſide, as if the motion had begun at the angle. The electric matter had however been evidently attracted by the veins of the cabbage leaf, having purſued them a little way, at leaſt having ſenſibly affected them, wherever it met with them in its paſſage.

THIS

THIS experiment ſuggeſted another, intended to determine whether the force of an exploſion was at all diminiſhed by being diverted from a right lined courſe, and made to turn in a great number of angles. To do this, I firſt found, by a great number of trials, what length of a ſmall iron wire I was able to melt with a battery of about twenty ſquare feet, in the middle of a circuit of about three yards of braſs wire, conſiderably thicker than the iron, and ſtretched in two right lines, ſuſpended on ſilken ſtrings. The length of the iron wire melted in theſe circumſtances was about three inches. I then took the ſame braſs wire, and, fixing pins into a board of baked wood, twiſted it about them, making it turn in a very great number of acute angles; and I put three inches of the ſame iron wire in the middle of this crooked circuit, that I had done in the ſtraight one, ſo that the electric matter in the exploſion was obliged to make a great number of turns at acute angles, before it could come to the iron wire; but I always found that the ſame length of iron wire was melted in theſe circumſtances, as in the other, and not the leaſt difference was perceived in the force.

BUT though the *form* of the wire through which an exploſion paſſed made no difference in its force, I found a very remarkable difference occaſioned by the *length of the circuit*, in wires of the ſame thickneſs, and which, I own ſurprized me very much.

IN

In order to aſcertain the practicability of firing mines by electrical exploſions, I took twenty-two yards of ſmall braſs wire (but ſo thick, however, that I could not have melted the leaſt part of it, by the force of any battery I have ever conſtructed) and extending it along a dry boarded floor, with a ſmall piece of iron wire, and a cartridge of gun powder about it, in the place that was moſt remote from the battery; I found that, upon the diſcharge, the wire was not melted, nor the gun powder exploded; alſo the report was very faint. In other circumſtances a charge of the ſame battery was able to melt more than nine inches of this iron wire, and this ſame cartridge was eaſily fired near the battery, connected with ſhorter pieces of the ſame braſs wire; ſo that the diminution of force muſt have been owing to the length of the circuit.

In the place of this ſmall braſs wire, I ſubſtituted an iron wire, one fifth of an inch thick; when about half an inch of the ſmall iron wire was exploded; ſo that the force was not leſſened ſo much in a circuit of the thick iron wire, as it had been in one of the ſmall braſs wire. In order to judge how much of the force might be loſt by nearer circuits, conſiſting of leſs perfect conductors, I joined the middle of the circuit made by the iron wire with water, in which both the wires were immerſed. The effect was, that the ſmall iron wire was only made red-hot, but not exploded as before.

Being ſenſible how much depended upon avoiding all leſſer circuits, whereby part of the fire of an exploſion might return to the battery, without reaching the extremity of the circuit, where I intended the whole of its force to be exerted; in the remaining experiments, I inſulated half the circuit of iron wire. There was no occaſion for inſulating the whole circuit: for if there was but one paſſage *to* or *from* the middle of it, there could be put one *from* or *to* it. In this method it was eaſy to aſcertain what loſs of force was occaſioned by the length of the circuit, as every other circumſtance was carefully excluded. And it preſently appeared to be very conſiderable; for though I could melt nine inches of the ſmall iron wire at the diſtance of fifteen yards from the battery, when I tried twenty yards, I found that I was juſt able to make ſix inches of it red-hot. The battery in theſe experiments was in the houſe, and the wires of which the circuit conſiſted were conveyed by ſilken ſtrings into a garden adjoining to the houſe.

Mentioning this loſs of force, occaſioned by the length of the circuit, in electrical exploſions to Dr. Franklin, he told me that the ſame obſervation had occurred to him, and that he had alſo been diſappointed in an attempt to fire gun powder at a diſtance from his battery.

Struck with this appearance, I endeavoured to aſcertain the quantity of this obſtruction, by trying what other courſes the electric

fire

fire would chuſe, preferably to a long metallic circuit. In the firſt place, taking about a yard of the ſmall braſs wire mentioned above, I diſpoſed it in the manner deſcribed [fig. 9. Pl. I.] connecting one of the ends with the outſide of the battery and the other with the inſide. In the firſt place, I brought the parts [*a*] and [*b*] (near the two extremities) into contact, and, upon the diſcharge, found there had been a fuſion in that place, and that a great part of the fire had taken the ſhorter circuit, though it had been obliged to quit the wire in one place, and enter in again in another. Afterwards I removed the parts [*a*] and [*b*] to a ſmall diſtance from one another, and, upon the exploſion, obſerved a ſtrong ſpark paſs between them. Removing them to greater and greater diſtances, I found the exploſion choſe to paſs above one third of an inch in the air, rather than make the circuit of the continued wire. Uſing a longer and ſmaller iron wire, the paſſage through the air exceeded half an inch. I then took four or five yards of iron wire, one tenth of an inch thick; when the paſſage through the air was ſtill half an inch; and taking three yards and a half of a wire that was one fifth of an inch thick, the ſpark in the air was half an inch, and ſometimes near three quarters of an inch. Making uſe of only half the length of this wire, the paſſage through the air was only half that diſtance, or one fourth of an inch. When I kept the place of near contact about the middle of this wire, and made the exploſion

ſion at the extremities of the whole wire, I was obliged to bring them about as near again, i. e. to little more than one eighth of an inch before the paſſage would be through the air; ſo that the force of the whole exploſion muſt have been greatly weakened by its paſſing through ſo much of the wire. Laſtly, I took a pair of kitchen tongs, the legs of which were two feet in length, and the ſmalleſt part of them above half an inch in diameter; when the circuit was made about one ſixth of an inch in the air (for at that diſtance from one another the ends of the tongs had been fixed) rather than through four feet of that thick iron.

NOTWITHSTANDING this paſſage of the exploſion through the air, at the ſame time that a metallic circuit was open for it, it was evident that the whole of the force did not paſs this way, nor indeed the greateſt part of it. For when I extended a ſmall iron wire between [*a*] and [*b*], I could only make about half an inch of it red-hot, whereas, when there was no other metallic circuit, I was able, with the ſame battery, to explode more than two inches of it.

As the electric fire meets with ſo much obſtruction in paſſing through a circuit of iron of this thickneſs, I make no doubt, but that it is conſiderably obſtructed in paſſing through metallic circuits of any thickneſs whatever; and that it would prefer a paſſage through the air, if they were made even of no great length. In this method, the different degrees of con-

ducting

ducting power in different metals may be tried, uſing metallic circuits of the ſame length and thickneſs, and obſerving the difference of the paſſage through the air in each. N. B. A common jar anſwers as well in theſe experiments as a large battery.

It is evident, from many experiments, that the whole fire of an exploſion does not paſs in the ſhorteſt and beſt circuit, but that if inferior circuits be open, part will paſs in them at the ſame time. Of this I made the following ſatisfactory trial. I took an iron chain, and laid it upon a table, in contact with a charged jar; ſo that the parts of it made two circuits for the diſcharge, which I could vary at pleaſure; and I obſerved that, when one of the circuits was but half an inch, and the other more than half a yard; yet, if the diſcharge was high, it always went in them both, there being conſiderable flaſhes between the links of the remoteſt part of the chain. If the charge was weak, it paſſed in the longer or metallic circuit only.

It is evident that when the wires of a battery are not in cloſe contact, there muſt be ſome loſs of force in the diſcharge; but this never appeared to me to be ſo conſiderable, as Mr. L. Epinaſſe ſeems to have imagined *. In order to aſcertain this by experiment, I firſt found, by repeated trials, what length of an iron wire, of a certain thickneſs, I was able to melt with a battery conſiſting of twenty

* Phil. Tranſ. Vol. lvii. pt. i. p. 186.

jars,

jars, conſtructed in the manner deſcribed above. It was about two inches and a half. I then ſoldered all the wires together, and alſo ſoldered one rod to them all, inſtead of a chain which I had uſed before, ſo that I avoided near a hundred ſparks, in each of which ſome force had been loſt; but I did not find, after many trials, that the power of the battery was ſenſibly diminiſhed. I ſtill could not melt three inches of the ſame wire.

SECTION XVI.

MISCELLANOUS EXPERIMENTS.

I. Observations on the electric spark taken through several pieces of metal.

MARCH the 24th, 1766. I obſerved that an electric ſpark taken from the prime conductor itſelf was not near ſo ſtrong and pungent, as one taken through a piece of metal inſulated, and interpoſed between my finger and the conductor.

The effect was the ſame whatever was the form of the interpoſed piece of metal. And, in this manner, whatever was preſented received a full and ſtrong ſpark; whereas a great part is commonly diſſipated, in pencils or ſtars, even when pretty large braſs knobs are preſented to the prime conductor itſelf, if the excitation be very powerful; unleſs both the conductor and knob have one preciſe degree of convexity, adapted to one another.

One ſingle braſs ball made the ſpark as ſtrong as the interpoſition of a long piece of metal, or of many pieces.

Whether one, a few, or a great number of pieces were uſed, it ſeemed that the intervals taken together muſt be equal.

But theſe intervals taken together will be larger when the pieces are placed in a right line, than when they are laid in a curve.

Whether one body, or a number of them be interpoſed; if a ſpark be ſolicited it will not ſtrike the firſt, unleſs it can, at the ſame time, ſtrike all the reſt.

All theſe experiments ſucceed, in the ſame manner, with the exploſion of a charged jar.

Some of the above mentioned circumſtances, I afterwards found, had been taken notice of by Signior Beccaria.

II. A deception relating to the direction of the electric spark.

As I was once amuſing myſelf with taking long ſparks from a large prime conductor of poliſhed copper, and conſidering the deceptions that electricians had fallen into with reſpect to the direction of the electric matter; I could not help being ſtruck with one deception, which the evidence of my ſenſes would never have rectified; and which ſhowed very clearly, how little the evidence of the ſenſes is to be depended upon in ſuch caſes. I obſerved, that, whether I made this large conductor give, or take the electric fire (for I could make it do either at pleaſure, and with the ſame force) I ſtill fancied that a ſpark taken with a braſs ball above the conductor deſcended to it, and that a ſpark taken below it aſcended from it; but ſparks taken laterally ſeemed to have no one certain direction.

III. An experiment intended to ascertain whether electric substances, in their natural state, contain more of the electric fluid than conductors.

Thinking to aſcertain Dr. Franklin's hypotheſis, concerning the eſſential difference between conductors and non-conductors, I made a pretty large piece of glaſs red-hot (in which ſtate I had proved it to be a real conductor of electricity) and placed it upon a ſmooth piece of copper, inſulated; ſuppoſing that, if electric ſubſtances had naturally a much greater ſhare of the electric fluid than conductors, this piece of glaſs, in paſſing from a conducting to a non-conducting ſtate, muſt exhauſt the copper of its natural ſhare of the electric fluid, and leave it electrified negatively. But I could perceive no kind of electricity, either in the copper, or the glaſs, during the whole time of its cooling.

Some

Some time after, I found that Mr. Cigna had endeavoured to aſcertain the ſame thing, by reducing ice into water; but ice and water are both conductors of electricity.

IV. The MUSICAL TONE of various discharges ascertained.

As the courſe of my experiments has required a great variety of electrical exploſions, I could not help obſerving a great variety in the muſical tone made by the reports. This excited my curioſity to attempt to reduce this variation to ſome meaſure. Accordingly, November the 17th, by the help of a couple of ſpinets, and two perſons who had good ears for muſic, I endeavoured to aſcertain the tone of ſome electric exploſions; and obſerved, that every diſcharge made ſeveral ſtrings, particularly thoſe that were chords to one another, to vibrate: but one note was always predominant, and ſounded after the reſt. As every exploſion was repeated ſeveral times, and three of us ſeparately took the ſame note, there remained no doubt but that the tone we fixed upon was, at leaſt, very near the true one. The reſult was as follows.

A jar containing half a ſquare foot of coated glaſs ſounded F ſharp, concert pitch. Another jar of a different form, but equal ſurface, ſounded the ſame.

A JAR of three ſquare feet ſounded C, below F ſharp. A battery, conſiſting of ſixty-four jars, each containing half a ſquare foot, ſounded F below the C.

THE ſame battery, in conjunction with another of thirty-one jars, each containing a ſquare foot, ſounded C ſharp. So that a greater quantity of coated glaſs, always gave a deeper note.

DIFFERENCES in the degree of a charge in the ſame jar made little or no difference in the tone of the exploſion; if any, a higher charge gave rather a deeper note.

FROM theſe experiments it will be eaſy for any perſon to compare the quantity of ſquare feet of coated glaſs, with the lengths of muſical ſtrings giving the ſame note. For this purpoſe, I could eaſily have found more terms of the ſeries; but I am afraid philoſophers in general will think it trifling enough to have found ſo many. I do not expect that electrical exploſions will ever be introduced into concerts of muſic; or that theſe experiments will be of any uſe to meaſure the extent of the clouds from which a clap of thunder proceeds. But true philoſophers will not abſolutely deſpiſe any new fact or obſervation, though it have no immediate, or apparent uſe.

V. Experiments on the effects of giving a METALLIC TINGE to the surface of glass.

It has long been a queſtion among electricians, where the electric matter that conſtitutes the charge of a plate of glaſs lies; whether within the pores of the glaſs, or only upon the ſurface; and ſome experiments I have made will perhaps be thought to throw ſome light on this difficult ſubject.

I considered that the common coating of a jar is not in actual contact with the glaſs, but that the metallic tinge, which is given to glaſs by an electric exploſion of the metal upon its ſurface, is probably in contact with it, if not lodged in its pores. I therefore gave a coating of this kind to both ſides of a plate of glaſs; and at firſt imagined that the glaſs coated in this manner did receive a charge, as well as if it had been coated in the common way; for it gave a real ſhock; but I very well remember, at that time, being a little ſurpriſed to ſee the electric fire run over the ſurface of that coating, a thing not poſſible in the common way. However, not ſufficiently attending to that circumſtance, I was purſuing the experiment, and trying whether, by combining this piece of glaſs with a large battery, and making the diſcharge of both upon this metallic tinge, I could not melt part of it, and thereby fetch it out of the glafs; as that method would have melted, and abfolutely difperfed a confiderable part of the coating of a common jar. But I was prodigioufly furprifed to find, that, though the connection of this metallic tinge with the battery was complete, the difcharge could not be made by bringing the difcharging rod upon it; though within three quarters of an inch of another brafs rod, that formed the communication between this plate of glafs and the battery. This convinced me that the metallic tinge did not anfwer the purpofe of a coating; and I prefently fatisfied myfelf, that a piece of uncoated glafs would receive juft fuch a charge as the tinged glafs had done.

To afcertain this matter ftill farther, I ftruck a tinge of this kind along two oppofite fides of a glafs tube, about half a yard in length; and holding it with my hand in contact with a part of this metallic tinge, found that it was excited juft like another tube: for when I difcharged the electricity of any part of the tube where the tinge was ftruck, it did not at all difcharge other parts of the tube, whither the fame tinge extended. Alfo the electric fnapping from the tinged part of the glafs could not be diftinguifhed from the fnapping at other places; except that, fometimes, where the gold lay thicker than ordinary, a denfer ftream of electric matter was vifible on its furface, and ran in feveral fmall ftreaks, in

different

different directions, from the place where the ſpark was taken.

This experiment ſeems to ſhow, that a coating of metal exceedingly near the ſurface of the glaſs is not at all affected either by the excitation or charging of it; and ſeems to confirm the hypotheſis of the electric fluid not entering the pores of the glaſs.

As the giving this metallic tinge to both ſides of a plate of glaſs is not very eaſy, the reader will not, perhaps, be diſpleaſed to be informed in what manner I ſucceeded in it. After fatiguing myſelf a long time in endeavouring to ſtrike a piece of leaf braſs into the two ſides of a plate of glaſs, to ſerve inſtead of a coating (having always broken the glaſs in fixing either the firſt or ſecond coating) I at length put two other pieces of glaſs, one on each ſide of that to which I intended to give the tinge, with pieces of leaf braſs between them both; and making one exploſion through both of them at the ſame time, the upper and the lower piece of glaſs were ſhattered to pieces, but the middle piece (being equally affected on both ſides) remained whole, and the coatings were nearly as I wiſhed them.

VI. An experiment intended to ascertain whether FERMENTATION contributes to the production of electricity.

September the 3d. In order to determine whether any of the electric fluid was discharged from, or acquired by bodies in a state of *fermentation*; I hung a pair of pith balls at the extremity of a piece of wire communicating with a quantity of steel filings, fermenting with oil of vitriol, inclosed in a glass vessel. But they never separated in the least.

VII. An experiment intended to ascertain whether EVAPORATION contributes to the production of electricity.

December the 26th. I put a small quantity of water upon a thin piece of glass, and made it all suddenly evaporate by a red-hot iron held under it; but the glass had acquired no degree of electricity. The weather was frosty.

VIII. An

VIII. An experiment intended to ascertain whether FREEZING be accelerated or retarded by electrification.

January the 6th, 1767. I expofed two difhes of water in the open air, while it was freezing intenfely, and electrified one of them pretty ftrongly; but could perceive no difference in the time, either when it began to freeze, which was in about three minutes, or in the thicknefs of the ice when both had been frozen fome time.

Happening to caft my eyes into the fields, out of the window, through which I had put the board which I ufed for the purpofe of this experiment, I obferved, on each fide of the electrified wire, the fame *dancing vapour*, which is feen near the furface of the earth in a hot fummer's day, or near any heated body that occafions an exhalation of vapours.

IX. The examination of a glass tube, which had been a long time charged and hermetically sealed.

December the 30th. I examined a glafs tube, about three feet in length, one half of which I had charged in the month of March preceding, and then fealed hermetically; but could not perceive that it was excited in the leaft leaſt degree, either by heating or cooling. The difference in the reſult of this experiment from ſeveral of Mr. Canton's, related vol. I. p. 346, I attribute to the thickneſs of the glaſs of my tube. Mr. Canton charged ſmall balls exceedingly thin. I alſo obſerved that there was no perceivable difference in the excitation of the charged or uncharged part of this tube, and that both parts acted exceeding well.

I AFTERWARDS opened this tube, and pouring a quantity of leaden ſhot into it, found it to contain a very good charge. It gave me one conſiderable ſhock, and ſeveral ſmall ones; as I made no uſe of an outward coating, but only diſcharged it by graſping it in ſeveral places by my hand.

X. THE WEIGHT REQUISITE TO BRING SOME BODIES INTO CONTACT ASCERTAINED BY THE ELECTRICAL EXPLOSION.

IT is plain from optical experiments, and alſo from a variety of other conſiderations, that bodies of no great weight lying upon one another, are not in actual contact. As the ſame thing is demonſtrated by an electric ſpark being viſible between pieces of metal lying upon one another, and other effects of electricity (particularly the fuſion of the parts through which it goes out of one body and enters another, not actually in contact with it) I was desirous to determine, by this criterion, what weight was sufficient to bring bodies into actual contact. With these views, I began with laying twenty smooth shillings upon one another, and making the discharge of the battery through them; thinking that the fusion would disappear, when the weight was sufficient to press them into contact. But I found that the whole column was not sufficient; for every piece was melted on both its sides, so that every two contiguous sides had spots exactly corresponding to one another. The deepest impressions were made near the top of the column, but they did not diminish with exact regularity. Perhaps small particles of dust might prevent some of them from coming sufficiently near one another.

AFTERWARDS, I gradually increased my weights, till I found that about six pounds was sufficient for my purpose. The fusion was visible under that weight, but never under above half a pound more, though I repeated the experiment several times.

I HAD some suspicion, that the largeness of the explosion might have occasioned a momentary repulsion, separation, and consequent fusion of these pieces of metal, though pressed by such a weight, but I found I was not able to produce any fusion; under a greater weight than that above mentioned, though, instead of thirty-two square feet of coated glass, I used above sixty.

XI. THE

XI. THE EFFECT OF THE ELECTRICAL EXPLOSION TRANSMITTED THROUGH VARIOUS LIQUORS.

I BELIEVE it is generally ſuppoſed, that ale, and other liquors are turned ſour by lightning, and I was deſirous of aſcertaining whether that fact (if it be one) was owing to the liquors being properly ſtruck with the lightning, or to the ſtate of the air, &c. during the thunder ſtorm. In order to this, I provided myſelf with a glaſs tube, nine inches long, and about a quarter of an inch in diameter, and by inſerting a wire into one end of it, which was ſtopped with ſealing-wax, could eaſily tranſmit an electrical ſhock through any ſubſtances contained in it.

BY this means, November the 13th, I began with diſcharging the exploſion of the battery through this tube, filled with *freſh ſmall beer*, and obſerved a conſiderable quantity of fixed air, or ſomething in the form of bubbles, to aſcend in it; but when I taſted it, I could perceive no difference between it and that out of which it was taken. No doubt the eſcape of ſo much air would tend to make it grow ſtale ſomething ſooner.

I THEN diſcharged ſeveral large ſhocks through a tube filled with *red wine*, but, after two or three days, could perceive no alteration in its taſte, or other ſenſible qualities. In this diſcharge, the electric matter did not imme-

immediately ſtrike the wine, but a metal rod, which juſt touched its ſurface; but I afterwards gave it two or three more ſhocks, in which the wine itſelf was made to receive the exploſion, but there was no variation in the effects.

I PASSED the ſhock through a tube filled with *milk*, in both the methods above mentioned; but it was ſweet three days after. Alſo a tube filled with *freſh ale* received ſeveral large ſhocks without undergoing any ſenſible change of properties.

IN all theſe exploſions I held the tube in my hand, without feeling any thing of the ſhock.

I ALSO made the electric ſpark viſible a great number of times in a ſmall quantity of *ſyrup of violets*, without producing any change of colour, or other ſenſible qualities.

XII. OBSERVATIONS ON THE COLOURS OF ELECTRIC LIGHT.

FINDING it advanced in the writings of ſeveral electricians (who muſt have copied it from one another, without ever repeating the experiment, though it may be done ſo ſoon) that electric light contained no priſmatic colours; I had the curioſity to try ſo extraordinary a fact, and immediately ſaw both the fallacy of the experiment when it was firſt made, and the cauſe of it. Holding a priſm before my eyes, while the electric ſparks were

were taken at the prime conductor, I observed as beautiful prismatic colours as any that are exhibited by the image of the sun; but when the light was a little diffused, as in those red or purple parts of a long spark, as it is called, the colours were not so vivid, and less easily distinguished from one another; and when the light was still more diffused, through a vacuum, the prism made no sensible alteration in the appearance of it. Thus the middle part of any large object appears of its natural colour through a prism: for though the rays be really separated, they are immediately confounded with others from different parts of the same object; so that its natural colour must necessarily be the result.

As the flames of different bodies yield very different proportions of the prismatic colours, I have often thought of attempting to ascertain the proportion of these colours in electric light, and compare it with the proportion of colours from light procured in various other ways; but I have not had leisure to pursue the inquiry.

The electric spark, taken in the middle of a phial filled with inflammable air, is always of a red or purple colour, and cannot be made to look white; but the larger the explosion is, the nearer it approaches to white.

I shall close this article with just mentioning another deception, which some persons may possibly lie under, with respect to what is called the *length of the electric spark.* When a jar is discharged, it may be imagined, that a body of fire is seen extending from the

the inſide to the outſide; whereas it is pretty certain, that that appearance is occaſioned by the very rapid motion of a ſingle ball of fire; in the ſame manner as a lighted torch, with no greater motion than a man's arm can give to it, will ſeem to make an entire circle of fire. That the fire of an electrical exploſion conſiſts of a ball, or cylinder, of no great length, ſeems pretty evident from one of the experiments with the circles, in which the diameter of the circle was the ſame, whether the exploſion was taken at the diſtance of half an inch, or of two inches; and alſo from the experiments of its paſſage over ſurfaces, in which it was ſometimes made twenty times longer than uſual, without any ſenſible diminution of its thickneſs.

XIII. Observations on the small wires that collect electricity from the excited globe.

Having made uſe of ſeveral braſs wires, about two inches and a half long, to collect the electric matter from my globe; I obſerved, after a month or two, that about half an inch of the ends of them, which touched the globe, had contracted a blackneſs, particularly on the ſide which lay next the globe. I then took them off the ring to which they had hung, and rubbing them carefully, obſerved, that the ſame friction which made the reſt of the wire quite bright, made but little alteration in this acquired blackneſs. Recollecting, at the

the ſame time, S. Beccaria's theory of magnetiſm, inſtead of replacing the wires, I hung two very fine needles in their place; and December the 20th, after about two months, in which I had made the moſt uſe of the machine, I examined them, and found that blackneſs at their points, but could not be ſure that they had acquired any degree of magnetiſm. They had, indeed, a very ſmall degree; but I had not examined them ſo very accurately before I hung them on, as I did afterwards. The experiment deſerves to be repeated with more care, but it requires a longer and more conſtant uſe of an electrical machine than, it is probable, I ſhall ever have an opportunity of employing.

XIV. Experiments intended to ascertain the difference in the conducting power of different metals.

In a converſation I once had with Dr. Franklin, Mr. Canton, and Dr. Price, I remember aſking whether it was probable that there was any difference in the conducting power of different metals; and if there was, whether it was poſſible to aſcertain that difference? I have ſince endeavoured to carry into execution a ſcheme propoſed by Dr. Franklin, viz. tranſmitting the ſame exploſion of the battery through two wires at the ſame time, of two different metals, and of the ſame thicknefs. They were hooked one to the other, and held faft in hand vifes, after they were meafured with a pair of compaffes to exactly the fame length. The experiments were much more pleafing and fatisfactory than I expected, but the refult by no means correfponded to my ideas *a priori*.

I FIRST joined a piece of iron wire, and a piece of copper wire. The explofion totally difperfed the iron, and left the copper untouched. The brafs likewife difappeared when joined with the copper, and the iron when joined with the brafs.

So far the experiments were extremely eafy; a fingle charge of the battery fufficing to determine the difference between any two; but when I came to compare the more perfect metals, I found much more difficulty, and was obliged to try four or five charges of the battery upon every two: for, their conducting powers being nearly the fame, I either made the charge too high, and difperfed them both; or too low, and touched neither of them. At length, I happened to hit upon fuch charges, that the copper vanifhed, and left both the filver and the gold; and the gold remained when the filver was difperfed. The hook, however, of the filver was melted off when the copper was difperfed, and the hook of the gold when the filver was difperfed: for the heat is always the greateft where the electric fire paffes from one body to another. Before the difperfion both of the copper and the filver, I had

 made

made explosions of such a strength, as though too small to melt them, gave them a blueish tinge.

From these experiments it is easy to settle the order in which the metals above mentioned are to be ranked, with respect to the power of electricity to melt them. It is as follows. *Iron, brass, copper, silver, gold.*

Not being able to get *lead* or *tin* drawn into wires, I got pieces of those metals rolled into plates equally thin, and taking small slips, of equal length and breadth, I transmitted the explosion through them; when the lead gave way the first. I intended to have compared these plates with others of iron, brass, &c. but had not an opportunity. I have little doubt but that tin would melt before iron; though indeed I had expected that tin would have melted before lead, and gold before silver. But according to Mr. Wilcke's experiments, lead is a worse conductor than any of the other metals. My own experiments on the circular spots, made me expect that gold would have melted before silver.

It is very remarkable, that when iron wire is melted by the electric explosion, *bright sparks* are generally dispersed about the room, in all directions; but that they are seldom, or never seen when wire of any other metal is used. If but a small residuum of a battery be taken between two iron rods, when the explosion is extremely little, a great number of small sparks will fly in all directions from the iron, to the distance of about an inch, and exhibit

exhibit a beautiful appearance. Fewer of theſe ſparks will be ſeen if one of the rods be braſs, and, I think, none in theſe ſmall diſcharges, if they both be braſs.

BEFORE any uſe can be made of theſe experiments, to determine the relative conducting power of the ſeveral metals, the order in which they melt with common heat ſhould be compared with the order in which they melt with the electrical exploſion; and the French tranſlation of this hiſtory places them in the following order; tin, lead, ſilver, gold, braſs, copper, iron. It is remarkable that iron ſhould require more heat to bring it into a ſtate of fuſion than any other metal, and yet ſhould require but a ſmall force of electricity to do it. Before this matter can be ſettled, it ſhould likewiſe be found, how much more eaſily any of the metals will be melted before another, by tranſmitting ſhocks through wires of different lengths and thickneſſes, which would be a very tedious buſineſs. I make no doubt but that an exploſion which melts a copper wire of any given diameter would diſperſe an iron wire of twice the diameter, ſo that copper would be a much greater ſecurity, as a conductor to guard a building from lightning than iron, beſides its being leſs liable to ruſt; but then it is more expenſive.

XV. Experiments with an ELECTRIFIED CUP.

I shall close the account of my experiments with a fmall fet, in which, as well as in the laft, I have little to boaft befides the honour of following the inftructions of Dr. Franklin. He informed me, that he had found cork balls to be wholly unaffected by the electricity of a metal cup, within which they were held; and he defired me to repeat and afcertain the fact, giving me leave to make it public.

Accordingly, December the 21ft. I electrified a tin quart veffel, ftanding upon a ftool of baked wood; and obferved, that a pair of pith balls, infulated by being faftened to the end of a ftick of glafs, and hanging intirely within the cup, fo that no part of the threads were above the mouth of it, remained juft where they were placed, without being in the leaft affected by the electricity; but that, if a finger, or any conducting fubftance communicating with the earth, touched them, or was even prefented towards them, near the mouth of the cup, they immediately feparated, being attracted to the fides; as they alfo were in raifing them up, the moment that the threads appeared above the mouth of the cup.

If the balls had hung in the cup a confiderable time without touching it, and they were

were taken out immediately after the electricity of the cup was difcharged, they were found to have acquired no degree of electricity.

If they had touched any part of the cup, though they fhowed no electricity while they were within it; yet, upon being taken out, they appeared to have acquired fome; which was more if they had touched a part near the edge of the cup, lefs if they had touched any part more remote from the edge, and leaft of all if they had touched the bottom only. If they had firft touched the fide near the top, and then the bottom, they came out with that fmall degree of electricity which they would have acquired, if they had touched the bottom only.

In any cafe, if the balls were taken out while the cup remained electrified, they neceffarily acquired fome degree of electricity, in paffing the mouth of the cup.

To purfue this experiment a little farther, I took a fmall coated phial, fuch as is reprefented upon the ftool [*c* Pl. II.] and obferved, that when I held it by the wire, within the electrified cup, it acquired no charge, the electricity of the cup affecting both the infide and outfide coating alike. If the external coating touched the bottom of the cup, the phial received a very fmall charge. If it was made to touch the fide, it acquired a greater charge; and the nearer to the top it was held,

held, the higher charge it received; the wire of the phial, which communicated with the inſide coating, being farther removed from the influence of the electricity of the cup.

May we not infer from this experiment, that the attraction of electricity is ſubject to the ſame laws with that of gravitation, and is therefore according to the ſquares of the diſtances; ſince it is eaſily demonſtrated, that were the earth in the form of a ſhell, a body in the inſide of it would not be attracted to one ſide more than another.

Doth it not follow from the experiments of the balls, compared with thoſe with the phial, that no body can receive electricity in one place, unleſs an opportunity be given for its parting with it in another; at leaſt, that a quantity muſt be repelled from any particular part before any more can enter, ſince a ſmall body can no more receive electricity when all its ſides are equally expoſed to the action of an electrified body, than a phial can be charged when both its coatings are equally expoſed to the ſame electricity?

Do not theſe experiments, likewiſe, favour the hypotheſis of S. Beccaria, that there is no electrical attraction without a communication of electricity?

Mr. Lullin alſo made ſeveral of theſe experiments with an electrified hollow veſſel.

fel. He obferved, that if an electrified cork ball, fufpended by a filken ftring, was let down into the veffel, and touched the bottom, it left all its electricity behind it. He alfo made the experiment with a glafs veffel, coated at the bottom and charged, with the fame event. It alfo made no difference whatever was the form or fize of the body let down into it, provided it was one third lefs than the depth of the veffel; but if, when it touched the bottom, it, at the fame time, reached the top, or came near the top of the veffel, it acquired electricity; and a confiderable degree, if it exceeded the top. The form of the veffel made no difference in his experiments, nor did it make any whether the veffel was intire or perforated. A wire net anfwered perfectly well. Thefe experiments, Mr. Lullin imagines, clearly prove Nollet's doctrine of the conftant motion of electric atmofpheres: for he thinks, that this free motion on which electrification depends, is prevented from taking place within the veffel, by the contrary tendency of the oppofite fides *.

Dr. Franklin, in the laft edition of his Letters, p. 326, fays, that poffibly the mutual repulfion of the oppofite fides of the

* Differtatio phyfica, p. 38.

electrified

electrified cup, may prevent the accumulation of the electric atmosphere upon them, and occasion it to stand chiefly on the outside. But he recommends it to the farther examination of the curious.

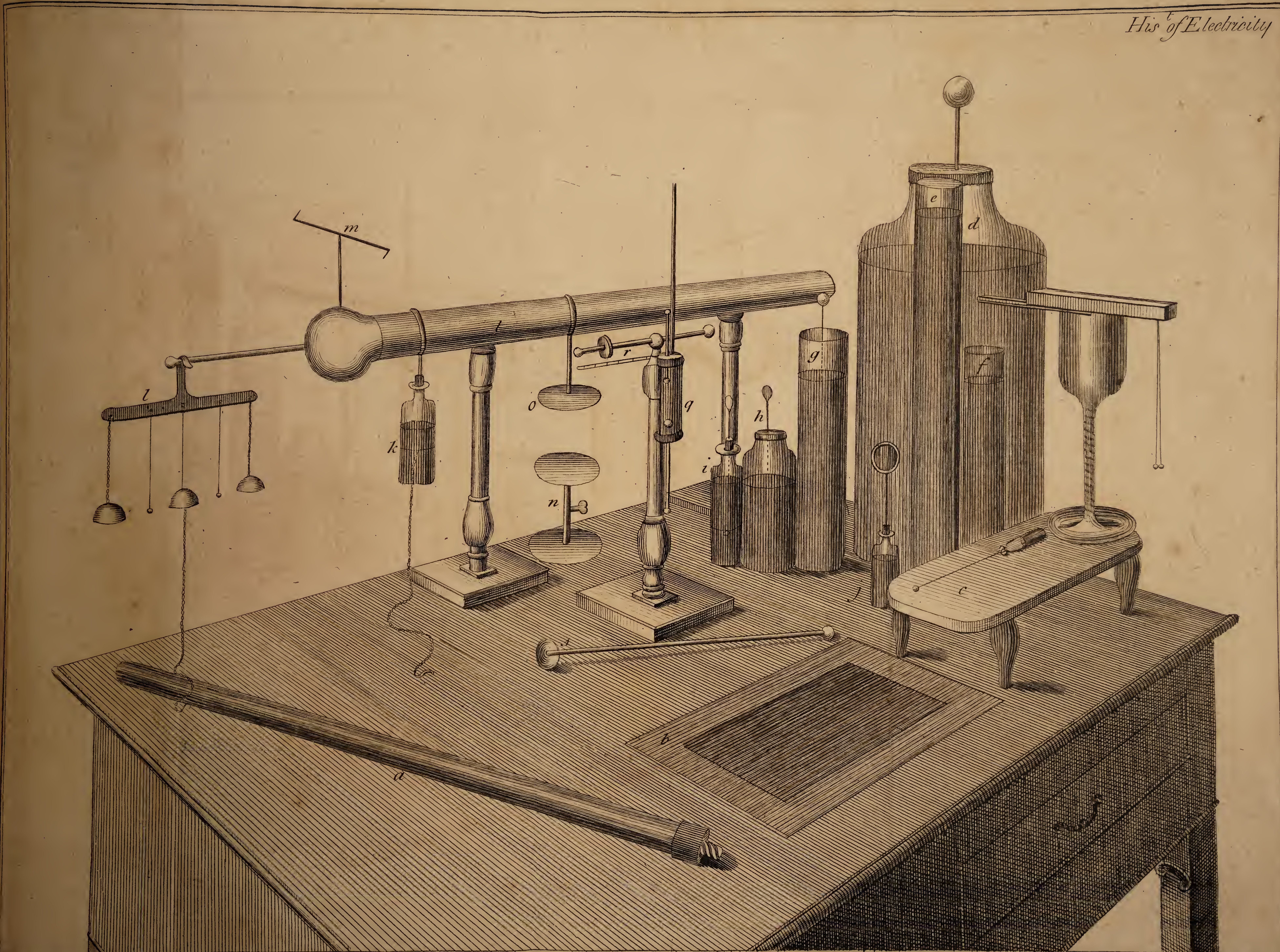

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PLATE III.

Hist. of Electricity

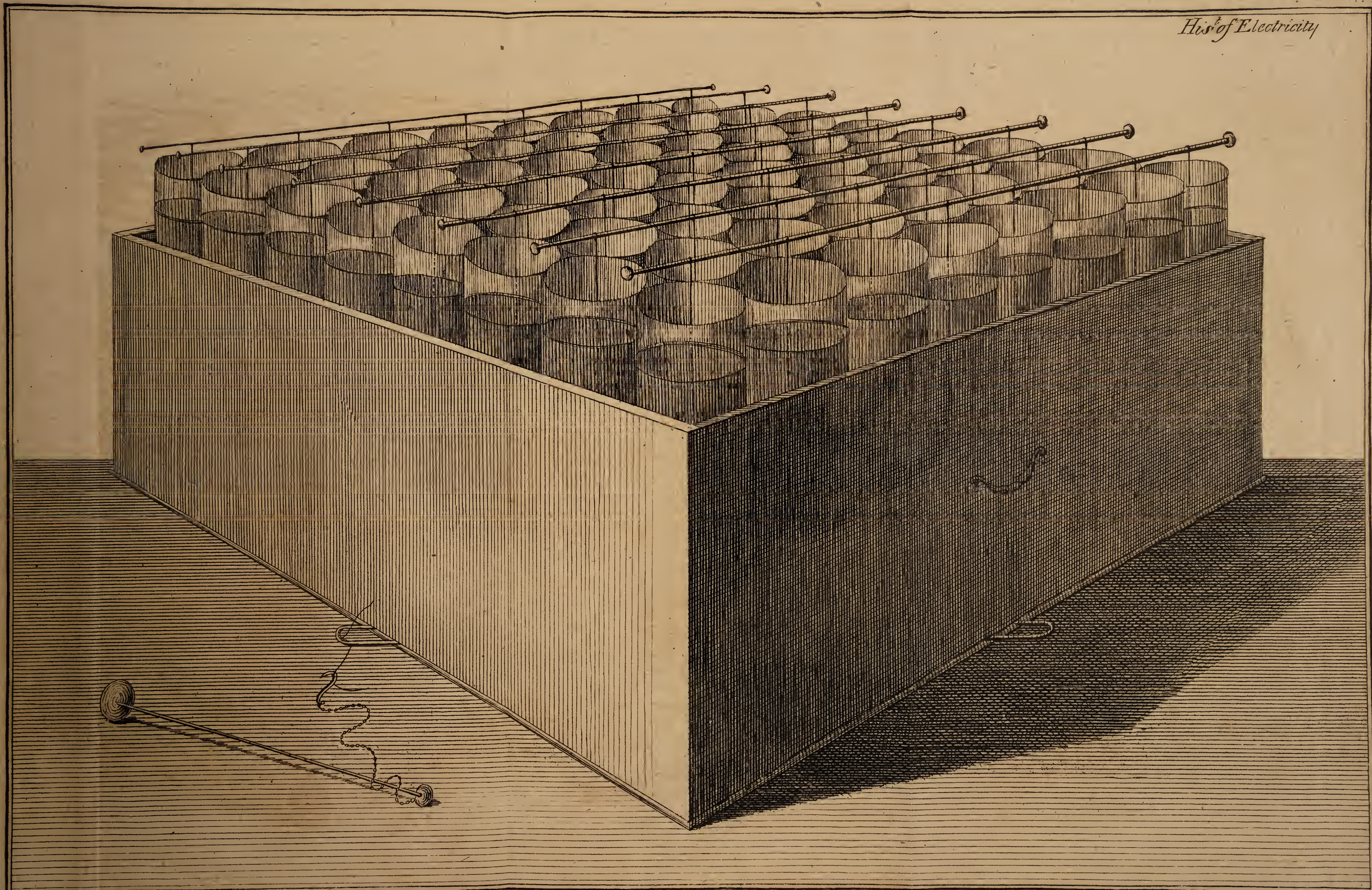

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Fig. 1.

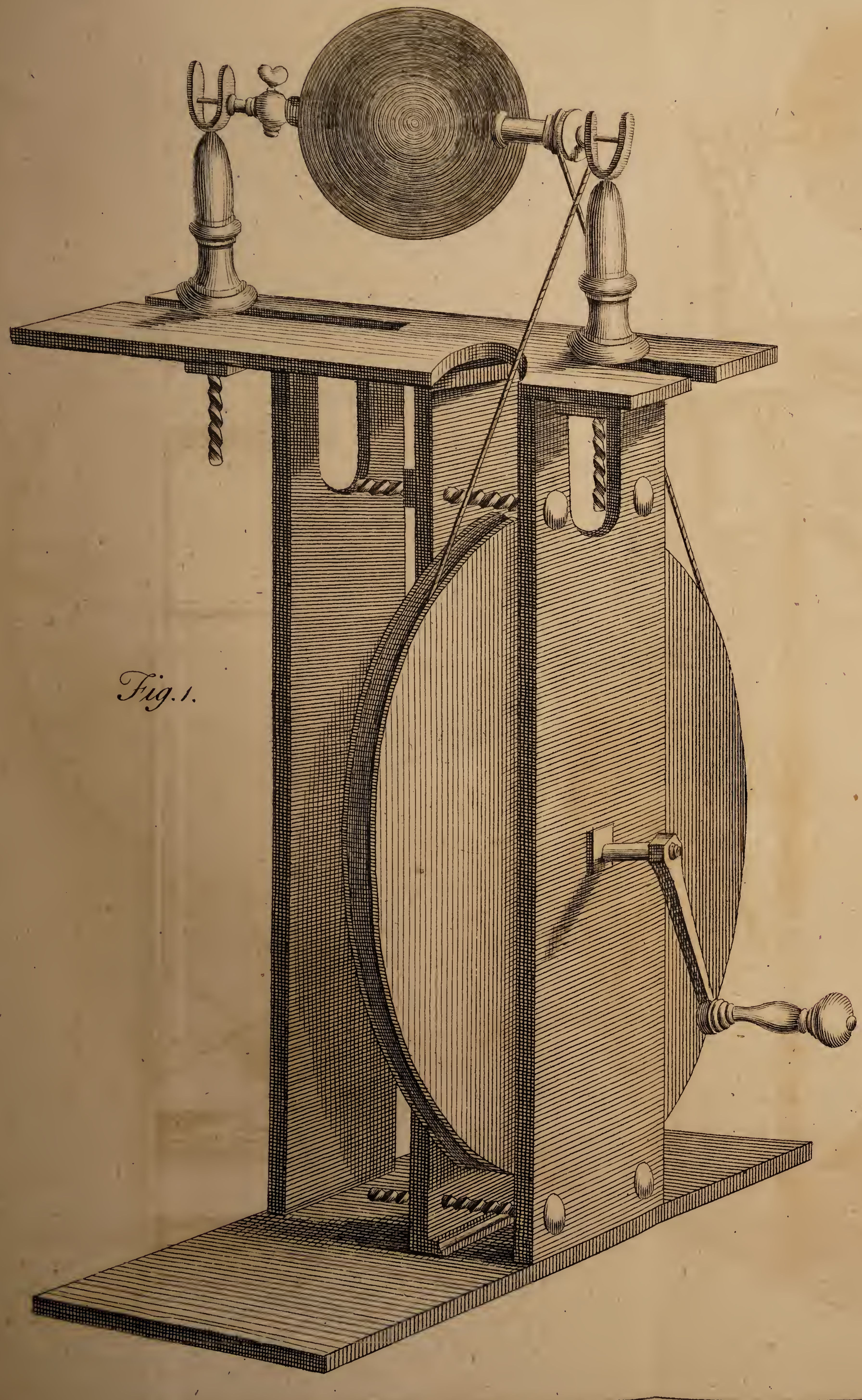

Fig. 2.

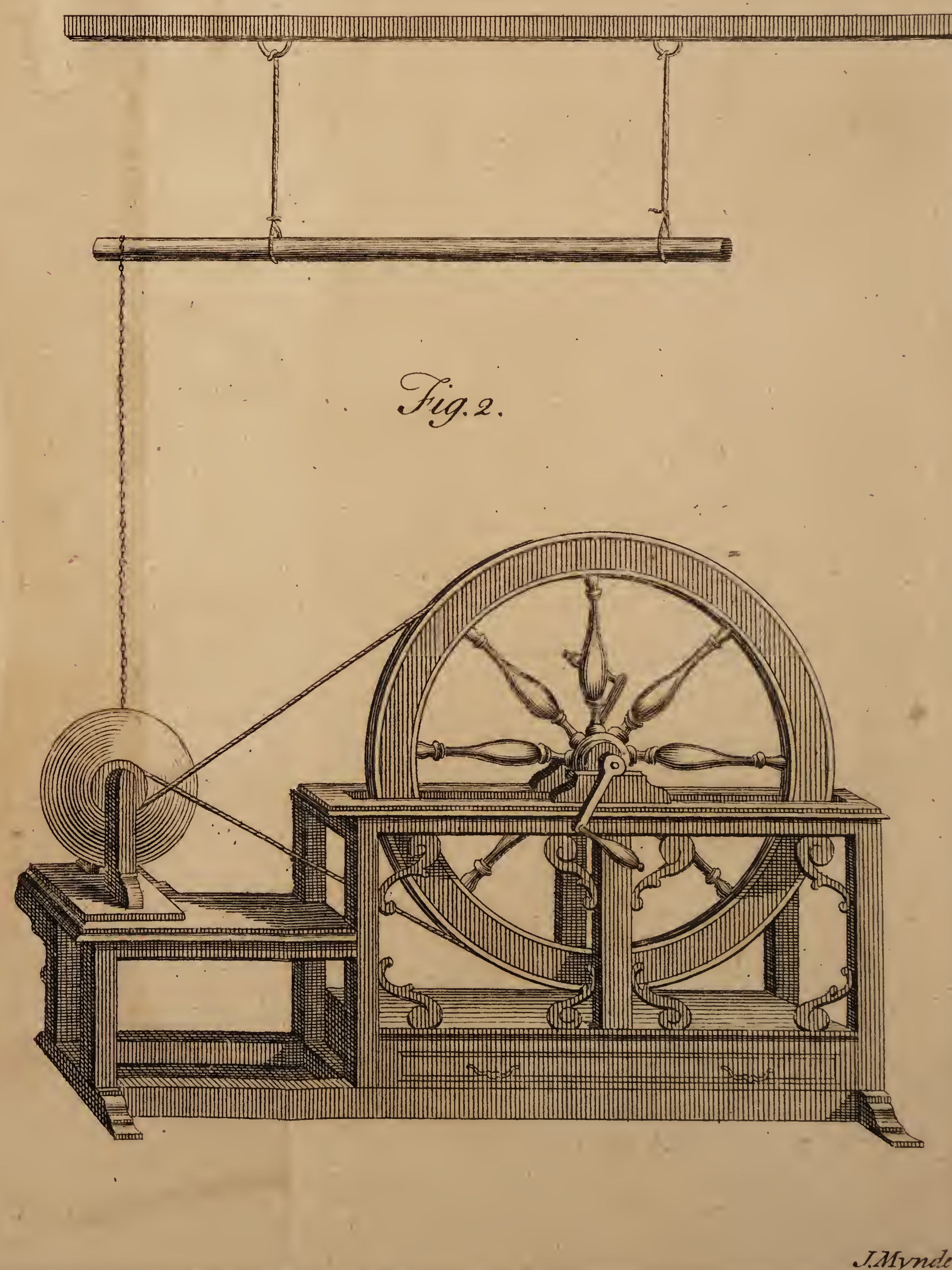

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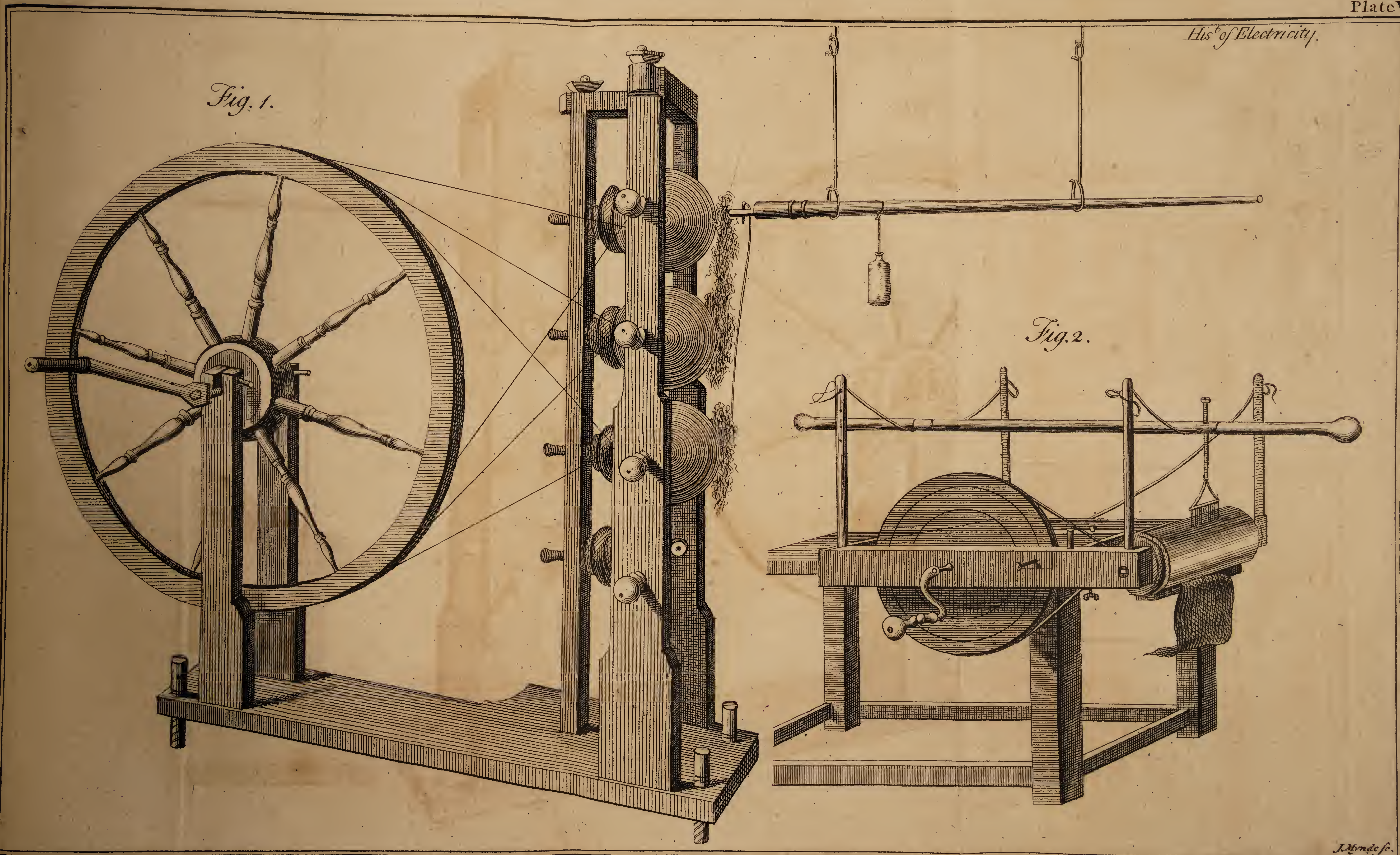
Plate V.



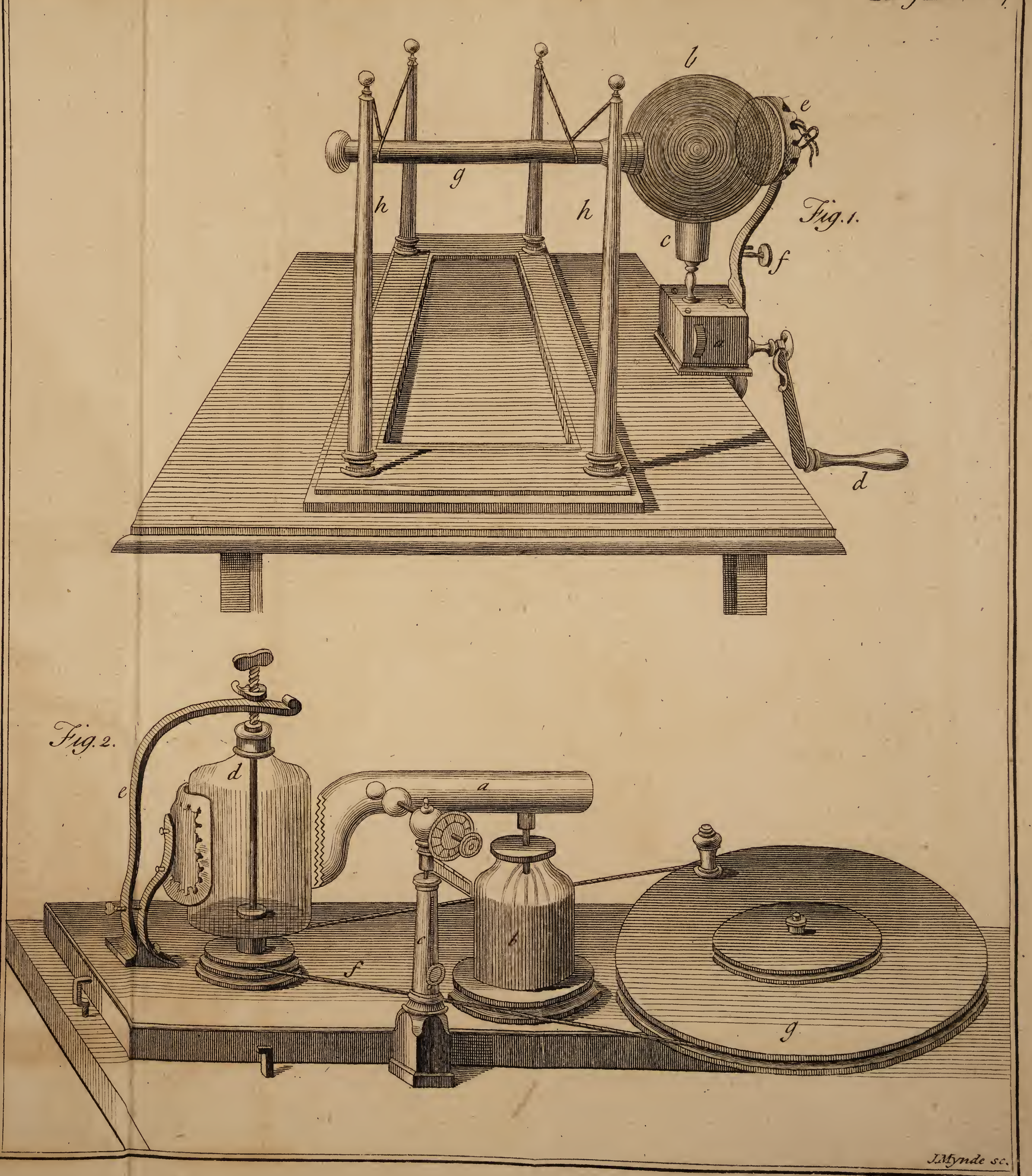


Fig. 1.
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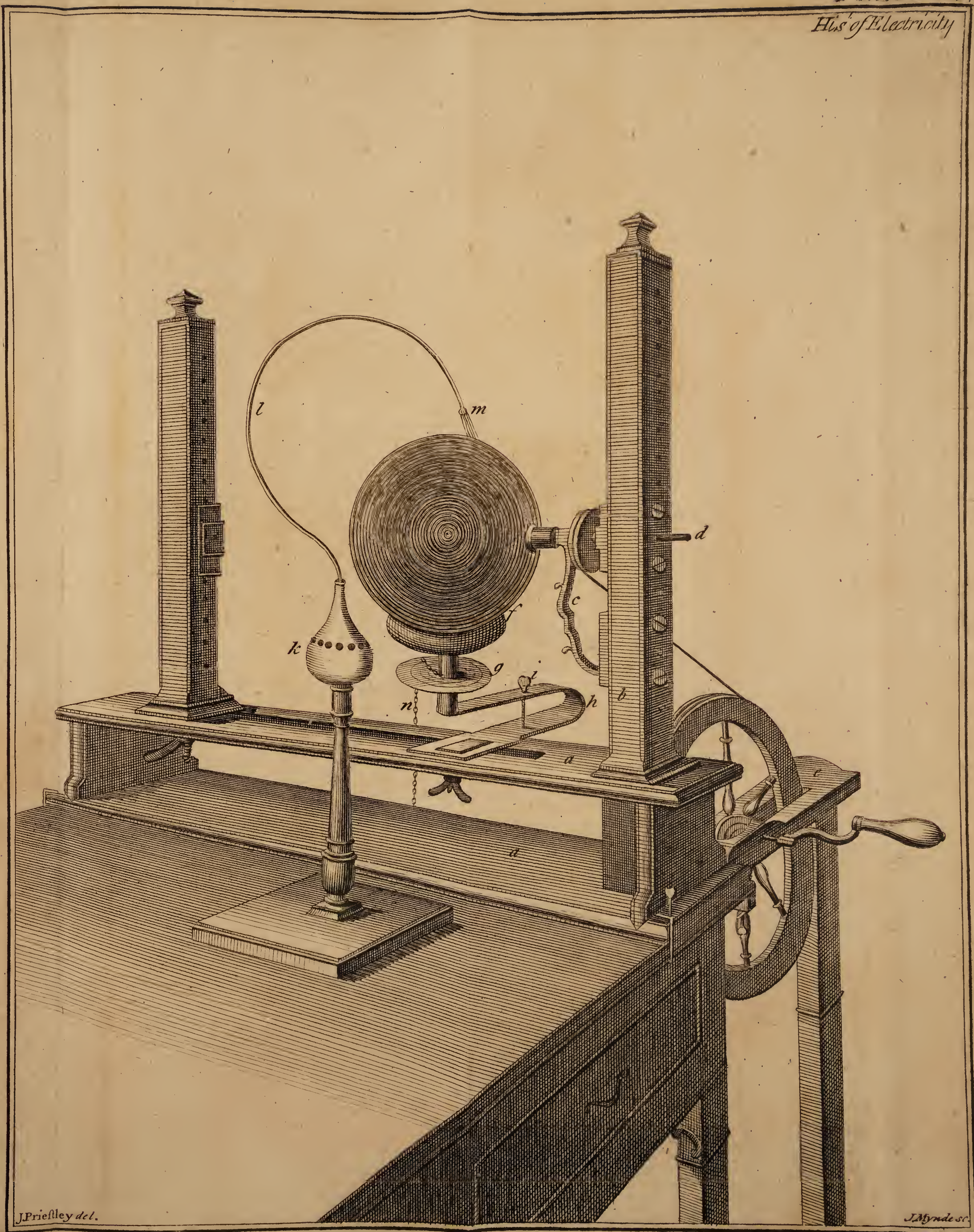
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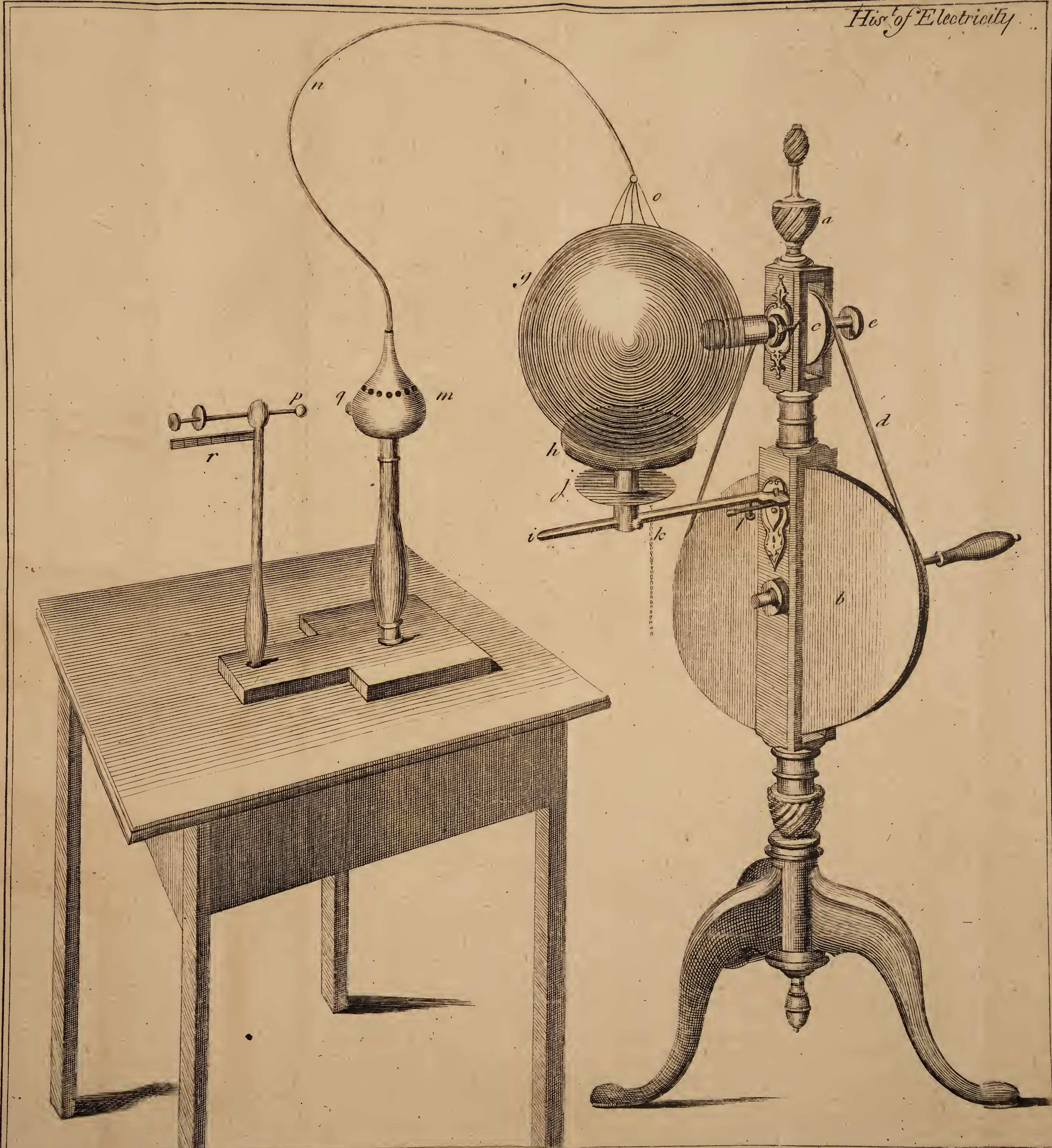

J. Priestley inv. & delin.

J. Mynde sculp.

A CATALOGUE OF BOOKS WRITTEN ON THE SUBJECT OF ELECTRICITY, EXCLUSIVE OF PAPERS IN BOOKS OF PHILOSOPHICAL TRANSACTIONS, AND OTHER MISCELLANEOUS WORKS; DISTINGUISHING [BY ASTERISMS] THOSE WHICH THE AUTHOR HAD SEEN, AND MADE USE OF IN COMPILING THIS WORK.

* GILBERT de Magnete Magneticisque corporibus, 1600, London, folio.

* *Otto de Guericke*'s experimenta nova Magdeburgica, 1672, Amsterdam, folio.

Haukesbee's physico mechanical experiments, 1709, 1719, London, octavo.

G. M. Boze's oratio inauguralis de electricitate, 2 parts, 1738, Wittemburg, Gralath's bibliothek.

J. Mortenson's dissertatio de electricitate, 1740, 1742, Upsal, quarto, Gralath's electritche bibliothek.

* *Desaguliers*' dissertation concerning electricity, 1742, London.

C. A. Hausenii novi profectus in historia electricitatis, Leipsic, 1743.

C. G. Kratzenstein's abhandlung von der nutzen der electricitat in der artzney wissenschafft, 2d. edit. 1745, Halle. Gralath's bibliothek.

A. G. Rosenberg's versuch einer erklarung von den ursachen der electricitat, 1745. Breslau. Gralath's bibliothek.

Nollet's conjettures sur les causes de l' elettricité des corps, Paris, 1745. Wilson, p. 12.

——— essai sur l' electricité des corps, 1746, 1754. Paris.

* *Waitz*'s abhandlung von der electricitat und deren ursachen, together with two other essays in High Dutch, and one in French, all written for the prize proposed by the academy at Berlin, 1745, Berlin, quarto.

* *Wilson*'s essay towards an explication of the phenomena of electricity deduced from the ether of Sir Isaac Newton, 1746, London, octavo.

* *Watson*'s experiments and observations, tending to illustrate the nature and properties of electricity, 3d edit. 1746, London, octavo.

* ——— sequel to the experiments and observations, tending to illustrate the nature and properties of electricity, 1746, 1747, London, octavo.

* *Freke*'s essay to show the cause of electricity, 1746, London, octavo.

Kratzenstein's theoria electricitatis, more geometrico explicata, Hall, 1746.

J. Piderit's dissertatio inauguralis de electricitate, 1745, Marburgh. Gralath's bibliothek.

* *Martin*'s essay on electricity, 1748, Bath, octavo.

Boze's recherches sur la cause, et sur la veritable theorie de l' electricité. 146

Conjetture fifiche intorna i fenomeni della machina elettrica, Rome, 1746, octavo. Hift. p. 168.

Francifco Pivati's lettere della elletricita medica, Venice, 1747.

Boze's tentamina electrica, Wittemberg, 1747.

* *Giambattifta Faure*'s conjetture fifiche intorno alle cagioni dé fenomeni offervati in Rome nella machina elettrica, 1747, Rome, quarto.

* *Rackftrow*'s mifcellaneous obfervations, together with a collection of experiments on electricity, 1748, London, octavo.

* *Watfon*'s account of experiments, made to difcover whether the electric power would be fenfible at great diftances, &c. 1748, London, octavo.

Recueil des traités fur l' electricité, traduits de l' Alemand et de l' Anglois, Paris, 1748, octavo, 3 vols.

* *Nollet*'s recherches fur les caufes particulieres des phenomenés electriques, 1749, Paris, twelves.

* *Jallabert*'s experiences fur l' electricité avec quelques conjectures fur la caufe de fes effets, 1749, Paris, twelves.

A Latin differtation on electricity by *P. Francois Plata*, a jefuit of Palermo, 1749. Hiftoire.

* *Wilfon*'s treatife on electricity, 1750, 1752, London, octavo. N. B. *This book is quoted in this work by the title of* Wilfon's Effay.

* *Secondat*'s hiftoire d' electricité in his Obfervations Phyfiques, 1750, Paris, twelves.

* *Boulanger*'s traité de la caufe et des phenomenes de l' electricité 1750, Paris, twelves. N. B. *I have feen no more than one part of this treatife, which I had of Dr.* Watfon. *How many more parts there are of the whole work, I have not been informed.*

Jof. Veratti obfervations fur l' electricité, aux quelles on a joint les experiences faites a Montpelier pour guerir les paralytiques au moyen de l'electricité, a la Haye, 1750, twelves.

Electricorum effectuum explicatio, by *Father Bina*, 1751.

* Hiftoire generale et particuliere de l' electricité, 1752, twelves.

J. H. Winckler's progr. de avertendi fulminis artificio ex doctrina electricitatis, Leipfic, 1753.

* *Giambattifta Beccaria* dell' elettricifmo artificiale e naturale, 1753, Turin, quarto.

S. H. Quelmalz differtatio de viribus electrices medicis, Leipfic, 1753, quarto.

* *Franklin*'s new experiments and obfervations on electricity, made at Philadelphia in America, part. 1. 3d. ed. 1760.

——————————— part. 2. 2d. ed. 1754.

——————————— part. 3. 1754, Lond. quarto.

* *Franklin*'s

* *Franklin*'s new experiments and obfervations on electricity, made at Philadelphia in America, new edition with additions, 1769.

Brevis relatio de electricitate propria lignorum, authore *P. Wimdelipo Ammersin* De Lucerne Helvetiorum. Ord. Minim St. Francifci de P. Conventual. 1755. Nollet's letters, vol. 2. p. 235.

J. B. Landriani differtatio de nova electricitatis theoria. Milan, 1755. Wilcke, 12.

J. A. Euleri, difquifitio de caufa phyfica electricitatis, ab academia fcientiarum imperiali Petropolitana præmio coronata, Peterfburg, 1755. Wilcke, 12.

* *Dalibard*'s hiftoire abrigée de l' electricité, and French verfion of Franklin's letters, 1766, Paris, 2 vols. twelves.

* *Lovet*'s fubtile medium proved, 1756, London, octavo.

* *Daniel Gralath*'s gefchichte der electricitat in the memoirs of the fociety at Dantzick, 1747, 1754, and 1756, Dantzig, quarto. N. B. Vol. 1ft. 2d. *and* 3d. *of this work, in the marginal references mean the volumes of the work in which it is contained.*

* Electrifche bibliothek. in the Dantzick memoirs for 1754, and 1756.

* *Johannes Carolus Wilcke*'s difputatio phyfica experimentalis de electricitatibus contrariis, 1757, Roftock, quarto. N. B. *This admirable treatife confifts of four parts, but, to my great regret, the copy which I had contained only the three firft of them. It was that which the author fent to Dr.* Franklin, *before the remainder was printed.*

Butfchany differtatio de fulgure et tonitru ex phenomenis electricis. Gottingen, 1757, quarto.

* *Giambattifta Beccaria*'s lettere dell' elettricifmo, 1758. Bologna, folio.

* *J. C. Wilcke*'s herrn Franklin's briefe von der electricitat nebft anmerkungen, 1758, Leipzig, twelves.

* *Wilfon* and *Hoadley*'s obfervations on a feries of electrical experiments, 2d. edit. 1759, London, quarto.

* *Æpinus*'s tentamen theoriæ electricitatis et magnetifmi, 1759, Peterfburg, quarto.

* *J. F. Hartmann*'s abhandlung von der verwandfchaft und æhnlichkeit der electrifchen kraft mit der erfcherechlichen luft erfcheinungen, von 1759, Hanover, octavo.

* *Nollet*'s lettres fur l' electricité, tome 1. 1749, tome 2. 1760, tome 3. 1767, Paris, twelves.

* *Du Tour*'s recherches fur les differens mouvements de la matiere electrique, 1760, Paris, twelves.

* *Wefley*'s defideratum, or electricity made plain and ufeful, 1760, London, twelves.

Æpinus on the tourmalin, 1762.

* *Nollet*'s Leçons de phyſique, tome 6th. 1764, Paris, twelves.
* *J. F. Hartmann*'s anmerkungen uber die nothigte achtſamkeit bey erforſchung der gewetter electricitat, &c. 1764. Hanover, quarto.
* *Johannes Franciſcus Cigna* de novis quibuſdam experimentis electricis, from the memoirs of the academy of Turin for the year 1765, quarto.
* *Lovet*'s philoſophical eſſays in three parts, 1766, London, octavo.
* *J. F. Hartmann*'s electriſche experimente in luftleeren raume, 1766, Hanover, twelves.
* *Amadeus Lullin*'s diſſertatio phyſica de electricitate, 1766, Geneva, octavo.

OTHER BOOKS, THE DATES OF WHICH I HAVE NOT FOUND.

Winkler's eſſai ſur la nature, la cauſe, et les effets de l' electricité. Hiſtoire, p. 33.
Boze's poem on electricity. Hiſtoire.
Obſervations ſur l' electricité par Mr.—— chirurgeon de la ſalpetre. Hiſt. p. 98.
Nouvelle diſſertation ſur l' electricité par un phyſicien de Chartres. Hiſtoire.
Mr. *Kruger*'s meditations on electricity. Hall.
Tentamen de vi electrica ejuſque phenomenis, auctore *Nic. Bammucaro.*
Laurentii Beraud S. J. theoria electricitatis, Peterſburg. Wilcke, p. 12.
Watkins on electricity.

As the reader will ſee by the aſteriſms what books I have had an opportunity of peruſing, he will ſee in what parts my hiſtory is moſt likely to be defective. And I ſhall think myſelf greatly obliged to any perſon, who will favour me with the uſe of any treatiſe which contains a diſcovery of importance. But I do not apprehend that any thing very material can have eſcaped me.

AN

I N D E X.

N. B. Whenever the ſecond volume is meant, it is particularly expreſſed thus, II. When no Roman figure is prefixed to the page, the firſt volume is always to be underſtood.

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